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The Bakerian Lecture for 1809.

On some new Electrochemical Researches on various Objects, particularly the metallic Bodies, from the Alkalies, and Earths, and on some Combinations of Hydrogen.

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From the Philosophical Magazine.

I. *Introduction.*

I HAVE employed no inconsiderable portion of the time that has elapsed, since the last session of the Royal Society, in pursuing the train of experimental inquiries on the application of Electricity to Chemistry, the commencement and progress of which this learned body has done me the honour to publish in their Transactions.

In this communication I shall, as formerly, state the results. I hope they will be found to lead to some views, and applications, not unconnected with the objects of the Bakerian Lecture: and though many of them are far from having attained that precision, and distinctness, which I could wish, yet still I flatter myself, that they will afford elucidations of some impor-

tant and abstruse departments of chemistry, and tend to assist the progress of philosophical truth.

II. *Some new Experiments on the Metals from the fixed Alkalies.*

In the paper in which I first made known potassium and sodium to the Royal Society, I ventured to consider these bodies according to the present state of our knowledge, as undecompounded, and potash and soda as metallic oxides, capable of being decomposed and recomposed, like other bodies of this class, and with similar phenomena.

Since that time, various repetitions of the most obvious of the experiments on this subject have been made in different parts of Europe. The generality of enlightened chemists have expressed themselves satisfied both with the experiments, and the conclusions drawn from them: but as usually happens in a state of activity in science, and when the objects of inquiry are new, and removed from the common order of facts, some inquirers have given hypothetical explanations of the phenomena, different from those I adopted.

MM. Gay Lussac and Thenard, as I have mentioned on a a former occasion, suppose potassium and sodium to be compounds of potash and soda with hydrogen; a similar opinion seems to be entertained by M. Ritter. M. Curaudau* affects to consider them as combinations of charcoal, or of charcoal and hydrogen, with the alkalies; and an Inquirer† in our own country regards them as composed of *oxygen* and *hydrogen*.

I shall examine such of those notions only as have been connected with experiments, and I shall not occupy the time of the society with any criticisms on matters of mere speculation.

In my two last communications, I have given an account of various experiments on the action of potassium upon ammonia, the process from which MM. Gay Lussac and Thenard derive their inferences. At the time that these papers were written, I had seen no other account of the experiments of the French chemists, than one given in a number of the Moniteur;

* *Journal de Physique*, June 1808.

† Nicholson's Journal, August 1809, p. 258.

and as this was merely a sketch, which I conceived might be imperfect, I did not enter into a minute examination of it. I have since seen a detail of their inquiry in the second volume of the Mem. d'Arcueil, a copy of which M. Berthollet has had the goodness to send me, and the publication of which is dated June 7, 1809: and from this detail, it seems that they still retain their opinion; but upon precisely the same grounds as those to which I have before referred. That no step of the discussion may be lost to the society, I shall venture to state fully their method of operation, and of reasoning.

They say that they heated potassium* in ammonia, and they found that a considerable quantity of ammonia was absorbed, and hydrogen produced; and that the potassium became converted into an olive-coloured fusible substance; by heating this substance strongly, they obtained three-fifths of the ammonia again, two-fifths as ammonia, one-fifth as hydrogen and nitrogen; by adding a little water to the residuum, they procured the remaining two-fifths, and found in the vessel in which the operation was carried on, nothing but potash.—Again, it is stated, that by treating a new quantity of metal with the ammonia disengaged from the fusible substance, they again obtained hydrogen, and an absorption of the ammonia; and by carrying on the operation, they affirm that they can procure from a given quantity of ammonia, more than its volume of hydrogen.

Whence, they ask, can the hydrogen proceed? Shall it be admitted that it is from the ammonia? But this, say they, is impossible; for all the ammonia is reproduced. It must then come from the water which may be supposed to be in the ammonia, or from the metal itself. But the experiments of M. Berthollet, jun. prove that ammonia does not contain any sensible quantity of water. Therefore, say they, the hydrogen gas must be produced from the metal; and as, when this gas is separated, the metal is transformed into potash, the metal appears to be nothing more than a combination of hydrogen, and that alkali.

It is obvious, that even supposing the statement of these gen-

* *Mem. d'Arcueil*, tom. ii. p. 309.

tlemen correct, their conclusions may easily be controverted. They affirm that all the ammonia is reproduced; but they do not obtain it without the addition of *water*. And of the oxygen which this would give to the potassium, and of the hydrogen which it might furnish, to reproduce the ammonia, they take no notice.

I have shown, by numerous experiments, many of which have been repeated before members of this society, that the results obtained, by applying heat to the fusible substance, are very different from those stated by the ingenious French chemists, when the operations are conducted in a refined and accurate manner.

In proportion as more precautions are taken to prevent moisture from being communicated to it, so in proportion is less ammonia regenerated; and I have seldom obtained as much as $\frac{1}{10}$ of the quantity absorbed. And I have never procured hydrogen and nitrogen, in the proportions in which they exist in ammonia; but there has been always an excess of nitrogen.

The processes which I have detailed in the last Bakerian lecture, and in the appendix to it, show this; and they likewise show that a considerable quantity of potassium is always revived.

I have lately performed the experiments, in a manner which I proposed, page 458 of the last volume of the Transactions, [Phil. Mag. vol. xxxiv. p. 344.] and the results have been very satisfactory; as far as they relate to the question of the nature of potassium.

I employed a tube of platina bored from a single piece, which having a stop-cock and adaptor of brass, connected with the mercurial apparatus, could be used as a retort; the potassium was employed in quantities of from three to four grains, and the absorption of the ammonia conducted as usual, in a retort of glass free from metallic oxides; and in a tray of platina.

In some of the processes, in which the heat was rapidly applied, some of the gray matter, which I have formerly described as a pyrophorus, passed over in distillation, and in these cases there was a considerable deficiency of hydrogen, as well

as nitrogen, in the results of the experiment; but when the heat was very slowly raised, the loss was much less considerable, and in several cases I obtained more than four-fifths of the potassium which had been employed; and very nearly the whole of the nitrogen, existing in the ammonia that had been acted upon.

I shall give an account of one process, conducted with scrupulous attention. The barometer was at 30·2^{in}, thermometer at 54° Fahrenheit. Three grains and a half of potassium were heated in 12 cubical inches of ammonia, 7·5 were absorbed, and 3·2 of hydrogen evolved. The fusible substance was not exposed to the atmosphere, but was covered with dry mercury, and immediately introduced into the tube; which, with its adaptors, was exhausted, and filled with hydrogen. They contained together $\frac{8}{10}$ of a cubical inch. The heat was very slowly applied by means of a fire of charcoal, till the tube was ignited to whiteness. Nine cubical inches of gas were given off, and $\frac{1}{2}$ of a cubical inch remained in the retort and adaptors. Of the 9 cubical inches, $\frac{1}{5}$ of a cubical inch was ammonia, 10 measures of the permanent gas, mixed with 7·5 of oxygen, and acted upon by the electrical spark, left a residuum of 7·5. The quantity of potassium formed, was such as to generate by its action upon water three cubical inches and $\frac{3}{10}$ of hydrogen gas.

Now if this experiment be calculated upon, it will be found, that 7·5—·2= to 7·3 of ammonia, by its electrical decomposition, would afford about 13·1 of permanent gas, containing 3·4 of nitrogen, and 9·7 of hydrogen. But the 3·2 cubical inches of hydrogen, evolved in the first part of the process, added to the 5·8 evolved in the second part of the process, =9; and the nitrogen in the 8·8 cubical inches of gas (or the 9—·2 of ammonia) will be about 3, and if we estimate ·34 of hydrogen, and ·16 of nitrogen, in the ·5, remaining in the retort, there will be very little difference in the results of the analysis of ammonia by electricity, and by the action of potassium; and calculating upon the $\frac{8}{10}$ of hydrogen preexisting in the tube and adaptors, the loss of hydrogen will be found proportionally rather greater than that of nitrogen.

In another experiment in which three grains of potassium were employed in the same manner, 6·78 cubical inches of am-

monia were found to be absorbed, and 2·48 of hydrogen only generated. The distillation was performed, the adaptors and tube being full of common air: 8 cubical inches of gas were produced; and there must have remained in the tubes and adaptors, the same quantity of residual air as in the process last described.

The 8 cubical inches of gas contained scarcely $\frac{2}{5}$ of a cubical inch of ammonia; and the unabsorbable part detonated with oxygen, in the proportion of 11 to 6, gave a residuum of 7·5.—The barometer was at 30·2^{in}, thermometer at 52^{0} Fahrenheit. Dr. Pearson, Mr. Allen, and Mr. Pepys were present during the whole of these operations, and kindly assisted in the progress of them.

Now 6·78—·4 of ammonia=6·38, and this quantity of gas decomposed by electricity, would afford 11·4 of permanent gas, consisting of 2·9 nitrogen, and 8·5 hydrogen; but there are produced in this experiment, of hydrogen, 2·48 in the first operation, and 4·28 in the second, and considering the nitrogen in the permanent gas as 3·32, ·8 must be subtracted for the common air; which would give 2·52 for the nitrogen generated; and to these must be added, the quantity of hydrogen and nitrogen in the tubes and adaptors.

The quantity of potassium regenerated was sufficient to produce 2·9 cubical inches of hydrogen.

In all experiments of this kind, a considerable quantity of black matter separated, during the time the potassium in the tube was made to act upon water.

This substance was examined. It was in the state of a fine powder. It had the lustre of plumbago, it was a conductor of electricity. When it was heated, it took fire at a temperature below ignition; and after combustion, nothing remained but minutely divided platina.

I exposed some of it to heat in a retort containing oxygen gas; there was a diminution of the gas; and a small quantity of moisture condensed on the upper part of the retort, which proved to be mere water.

I made two or three experiments, with a view to ascertain the quantity of this substance formed, and to determine more fully its nature. I found that in the process in which from three

to four grains of potassium were made to act upon ammonia in a vessel of platina, and afterwards distilled in contact with platina, there were always from four to six grains of this powder formed; but I have advanced no further in determining its nature, than in ascertaining, that it is platina combined with a minute quantity of matter, which affords water by combustion in oxygen.

In the processes on the action of potassium and ammonia, in which iron tubes were used, as appears from the experiments detailed in the last Bakerian lecture, and the appendix, there is always a loss of nitrogen, a conversion of a portion of potassium into potash, and a production of hydrogen. When copper tubes are employed, the hydrogen bears a smaller proportion to the nitrogen, and more potassium is revived.

In these experiments, in which platina has been used, there is little or no loss of potassium or nitrogen; but a loss smaller or greater of hydrogen.

It will be asked, on what do these circumstances depend? Does the affinity of certain metals for potassium prevent it from gaining oxygen from ammonia, and do platina and copper combine with a small quantity of hydrogen, or its basis? Or are there some sources of inaccuracy in those processes, in which nitrogen has appeared to be decomposed? The discussion of these difficult problems will be considered in that part of this lecture, in which the nature of ammonia will be illustrated by some new experiments. The object of the present part of the inquiry is the demonstration of a part of chemical doctrine, no less important and fundamental to a great mass of reasoning, namely, that by the operation of potassium upon ammonia, it is not *a metallic* body that is decompounded but the volatile alkali, and that the *hydrogen* produced does not arise from the potassium, as is asserted by the French chemists, but from the *ammonia*, as I have always supposed; the potassium in the most refined experiments is *recovered*, but neither the ammonia nor its elements can be reproduced, except by introducing a new body, which contains oxygen and hydrogen.

I have made an experiment upon the action of sodium on ammonia, with the same precautions as in the experiments

just detailed, a tray, and the same tube of platina being employed.

3·$\frac{3}{10}$ grains of sodium I found absorbed 9·1 of ammonia, and produced about 4·5 of hydrogen, and the fusible substance, which was very similar to that from potassium, distilled, did not give off $\frac{1}{20}$ of the ammonia that had disappeared; and this small quantity I am inclined to attribute to the presence of moisture. The permanent gas produced, equalled twelve cubical inches, and, by detonation with oxygen, proved to consist of nearly two of hydrogen to one of nitrogen. Sodium was regenerated, but an accident prevented me from ascertaining the quantity.

Whoever will consider with attention, the mere visible phenomena of the action of sodium on ammonia, cannot, I conceive, fail to be convinced that it is the volatile alkali, and not the metal, which is decompounded in this process.

As sodium does not act so violently upon oxygen, as potassium; and as soda does not absorb water from the atmosphere, with nearly so much rapidity as potash, sodium can be introduced into ammonia, much freer from moisture, than potassium. Hence, when it is heated in ammonia, there is no effervescence, or at least one scarcely perceptible. Its tint changes to bright azure, and from bright azure to olive green, it becomes quietly and silently converted into the fusible substance, which forms upon the surface, and then flows off into the tray. It emits no elastic fluid, and gains its new form, evidently, by combining with one part of the elementary matter of ammonia, whilst another part is suffered to escape in the form of hydrogen.

It will not be necessary for me to enter into a very minute experimental examination of the opinion of M. Curaudau, that the metals of the alkalies are composed of the *alkalies* merely united to *charcoal;* the investigation upon which he has founded his conclusions, is neither so refined, nor so difficult, as that which has been just examined. This gentleman has been misled by the existence of charcoal, as an accidental constituent in the metals he employed, in a manner much more obvious, than that in which MM. Gay Lussac and Thenard have been misled by the moisture which interfered with their results.

M. Curaudau states, that when sodium is oxidated, carbonic

acid is formed. This I have never found to be the case, except when the sodium was covered by a film of naphtha. I burnt two grains of sodium in eight cubical inches of oxygen: nearly two cubical inches of oxygen were absorbed, and soda in a state of extreme dryness, so that it could not be liquefied by a heat below redness, formed. This soda did not give out an atom of carbonic acid, during its solution in muriatic acid. Three grains of sodium were made to act upon water; they decomposed it with the phenomena which I have described in the Bakerian lecture for 1807. Nearly six cubical inches of hydrogen were produced. No charcoal separated; no carbonic acid was evolved, or found dissolved in the water. Whether the metals of potash or soda were formed by electricity, or by the action of ignited iron on the alkalies, the results were the same. When charcoal is used in experiments on potassium or sodium, they usually contain a portion of it in combination; and it appears from M. Curaudau's method of decomposing the alkalies, that his metals must have been carburets, not of potash and soda, but of potassium and sodium.

M. Ritter's argument, in favour of potassium and sodium being compounds of hydrogen, is their extreme lightness. This argument I had in some measure anticipated, in my paper on the decomposition of the earths; no one is more easily answered. Sodium absorbs much more oxygen than potassium, and on the hypothesis of hydrogenation, must contain much more hydrogen; yet though soda is said to be lighter than potash, in the proportion of 13 to 17 nearly,* yet sodium is heavier than potassium in the proportion of 9 to 7 at least.

On the theory which I have adopted, this circumstance is what ought to be expected. Potassium has a much stronger affinity for oxygen than sodium; and must condense it much more, and the resulting higher specific gravity of the combination is a necessary consequence.

M. Ritter has stated, that of all the metallic substances he tried for producing potassium by negative Voltaic electricity, tellurium was the only one by which he could not procure it. And he states the very curious fact, that when a circuit of elec-

* *Hassenfratz, Annal. de Chim.* tome xxviii. p. 11.

tricity is completed in water, by means of two surfaces of tellurium, oxygen is given off at the positive surface, no hydrogen at the negative surface, but a brown powder, which he regards as a hydruret of tellurium, is formed and separates from it; and he conceives that the reason why tellurium prevents the metallization of potash is, that it has a stronger attraction for hydrogen than that alkali.

These circumstances of the action of tellurium upon water, are so different from those presented by the action of other metals, that they can hardly fail to arrest the attention of chemical inquirers. I have made some experiments on the subject, and on the action of tellurium on potassium, and I find that instead of proving that potassium is a compound of potash and hydrogen, they confirm the idea of its being as yet like other metals undecompounded.

When tellurium is made the positive surface in water, oxygen is given off; when it is made the negative surface, the Voltaic power being from a battery composed of a number of plates exceeding 300, a purple fluid is seen to separate from it, and diffuse itself through the water; the water gradually becomes opake and turbid, and at last deposits a brown powder. The purple fluid is, I find, a solution of a compound of tellurium and hydrogen in water; which, in being diffused, is acted upon by the oxygen of the common air, dissolved in the water, and gradually loses a part of its hydrogen, and becomes a solid hydruret of tellurium. The compound of hydrogen and tellurium produced at the negative pole when uncombined is gaseous at common temperatures, and when muriatic acid or sulphuric acid are present in the water, it is not dissolved, but is given off, and may be collected and examined.

I acted upon potash by means of a surface of tellurium, negatively electrified, by a part of the large Voltaic apparatus lately constructed on a new plan in the laboratory of the Royal Institution, an account of which, with figures, will be found annexed to this paper. 1000 double plates were used. The potash was in the common state, as to dryness. There was a most violent action, and a solution of the tellurium, with much heat, and a metallic mass, not unlike nickel in colour, was formed; which when touched by water, did not inflame nor effervesce,

but rendered the water of a beautiful purple colour, and when thrown into water entirely dissolved, making a bright purple tincture. It immediately occurred to me, that the whole of the hydrogen, which in common cases would have been furnished from the decomposition of the water, had in this instance combined with the tellurium, and that the *telluretted* hydrogen (if the name may be used) had formed with the oxidated potassium, *i. e.* the potash, a peculiar compound, soluble in water; and this I found to be the case; for on pouring a little diluted muriatic acid into the mixture, it effervesced violently, and gave a smell very like that of sulphuretted hydrogen; metallic tellurium was formed where it came in contact with the air, and muriate of potash was found dissolved in the mixture.

It seemed evident from this fact, that in the action of tellurium negatively electrified upon potash, potassium was produced as in all other cases, and that it combined with the tellurium, and formed a peculiar alloy; and this opinion was further confirmed, by the immediate action of potassium upon tellurium. When these metals were gently heated in a retort of green glass, filled with hydrogen gas, they combined with great energy, producing most vivid heat and light, and they composed an alloy of a dark copper hue, brittle, infusible at a heat below redness, and possessing a crystalline fracture. When the tellurium was in excess in this mixture, or even nearly equal to the potassium in quantity, no hydrogen was evolved by the action of the alloy upon water; but the compound of telluretted hydrogen and potash was formed, which remained dissolved in the fluid, and which was easily decomposed by an acid.

The very intense affinity of potassium and tellurium for each other, induced me to conceive that the *decomposition of potash* might be easily effected, by acting on the oxide of tellurium and potash at the same time, by heated charcoal; and I soon proved that this was the case. About 100 grains of oxide of tellurium, and 20 of potash, were mixed with 12 grains of well burnt charcoal in powder, and heated in a green glass retort; before the retort became red there was a violent action, much carbonic acid was given off, a vivid light appeared in the retort, and there was found in it the alloy of tellurium and potassium.

In attempting to reduce some oxide of tellurium by charcoal, which Mr. Hatchett had the kindness to give me for the purposes of these experiments, and which must have been precipitated by potash, or from a solution in potash, I found that a sufficient quantity of alkali adhered to it, even after it had been well washed, to produce an alloy of potassium and tellurium; but in this alloy the potassium was in very small quantity. It was of a steel gray colour, very brittle, and much more fusible than tellurium.

I shall not arrest the progress of discussion, by entering at present into a minute detail of the properties of the *aëriform* compound of tellurium and hydrogen; I shall mention merely some of its most remarkable qualities, and agencies, which, as will be shown towards the close of this paper, tend to elucidate many points immediately connected with the subject in question. The compound of tellurium and hydrogen is more analogous to sulphuretted hydrogen, than to any other body. The smell of the two substances is almost precisely the same.* Its aqueous solution is of a claret colour; but it soon becomes brown, and deposits tellurium, by exposure to air. When disengaged from an alkaline solution by muriatic acid, it reddens moistened litmus; but after being washed in a small quantity of water, it loses this property; but in this case likewise it is partially decomposed by the air in the water; so that it is not easy to say, whether the power is inherent in it, or depends upon the diffusion of a small quantity of muriatic acid through

* In some experiments, made on the action of tellurium and potassium, in the laboratory of my friend John George Children, Esq. of Tunbridge, in which Mr. Children, Mr. Pepys, and Mr. Warburton cooperated, the analogy between the two substances struck us so forcibly, as for some time to induce us to conceive that *tellurium* might contain *sulphur*, not manifested in any other way but by the action of Voltaic electricity, or by potassium; and some researches made upon the habitudes of different metallic sulphurets, at the Voltaic negative surface, rather confirmed the suspicion; for most of the sulphurets that we tried, which were conductors of electricity, absorbed hydrogen in the Voltaic circuit. The great improbability, however, of the circumstance that sulphuric acid, or sulphur in any state of oxygenation could exist in a metallic solution, which was not manifested by the action of barytes, induced me to resist the inference; and further researches, made in the laboratory of the Royal Institution, proved that the substance in question was a new and singular combination.

it. In other respects, it resembles a weak acid, combining with water, and with the alkalies. It precipitates most metallic solutions. It is instantly decomposed by oxymuriatic acid, depositing a film, at first metallic, but which is soon converted into muriate of tellurium.*

As arsenic has an affinity for hydrogen, it occurred to me as probable, that it would present some phenomena analogous to those offered by tellurium, in its action upon potassium, and in its operation upon water, when electrified.

Arsenic made the negative surface, in water, by means of a part of the new battery, containing 600 double plates, became dark-coloured, and threw down a brown powder; but it likewise gave off a considerable quantity of inflammable gas.

Arsenic negatively electrified in a solution of potash, likewise afforded elastic matter; but in this case the whole solution took a deep tint of brown, and was pellucid; but it became turbid, and slowly deposited a brown powder, by the action of an acid. When arsenic was made the negative surface, in contact with solid potash, an alloy of arsenic and potassium was formed of a dark gray colour, and perfectly metallic; it gave off arseniuretted hydrogen by the action of water with inflammation, and deposited a brown powder.

When potassium and arsenic† were heated together in hydrogen gas, they combined with such violence as to produce the phenomena of inflammation, and an alloy was produced of the same kind as that formed by means of the Voltaic battery.

* From the results of one experiment which I tried, it seems that tellurium, merely by being heated strongly in dry hydrogen, enters into combination with it. An accident prevented me from ascertaining whether the compound so formed, is exactly the same as that described in the text.

† In reasoning upon the curious experiment of Cadet, of the production of a volatile pyrophorus by the distillation of acetite of potash, and white oxide of arsenic, Fourcroy Connais Chem. tom. viii. p. 197, I conceived it probable, that this pyrophorus was a volatile alloy of potassium and arsenic. But from a repetition of the process I find, that though potash is decompounded in this operation, yet that the volatile substance is not an alloy of potassium, but contains charcoal and arsenic, probably with hydrogen. The gases not absorbable by water given off in this operation, are peculiar. Their smell is intensely fetid. They are inflammable, and seem to contain charcoal, arsenic, and hydrogen: whether they are mixtures of various gases, or a single compound, I am not at present able to decide.

As tellurium and arsenic both combine with hydrogen, it appeared to me probable, that by the action of alloys of potassium, with tellurium and arsenic, upon ammonia, some new phenomena would be obtained, and probably, still further proofs of the decomposition of the volatile alkali, in this process afforded; and this I found was actually the case.

When the easily fusible alloy of tellurium with potassium, in small quantity, was heated in ammonia, the surface lost its metallic splendor, and a dark brown matter was formed, which gave ammonia by exposure to air; and the elastic fluid, which was generated in this operation, consisted of four-sixths nitrogen, instead of being pure hydrogen, as in the case of the action of potassium alone.

The alloy of arsenic and potassium, by its action upon ammonia, likewise produced a gas which was principally nitrogen; so that if it be said that the metal, and not the volatile alkali, is decomposed in processes of this kind, it must be considered in some cases as a compound of nitrogen, and in others a compound of hydrogen; which are contradictory assumptions.

None of the chemists who have speculated upon the *imaginary hydrogenation* of potash, as far as my knowledge extends, have brought forward any arguments of analysis, or synthesis. Their reasonings have been founded, either upon distant analogies, or upon experiments in which agents, which they did not suspect were concerned. No person, I believe, has attempted to show that when potassium or sodium is burnt in oxygen gas, water is formed, or that water is generated when potassium decomposes any of the acids;* and no one has been able to form potassium, by combining hydrogen with potash. I stated

* When in October 1807, I obtained a dark-coloured combustible substance from boracic acid, at the negative pole in the Voltaic circuit, I concluded that the acid was probably decomposed, according to the common law of electrical decomposition. In March 1808, I made further experiments on this substance, and ascertained that it produced acid matter by combustion; and I announced the decomposition in a public lecture delivered in the Royal Institution March 12. Soon after I heated a small quantity of potassium, in contact with dry boracic acid, no water was given off in the operation, and I obtained the same substance as I had procured by electricity. MM. Gay Lussac and Thenard have likewise operated upon boracic acid, by potassium, and they conclude that they have decompounded it; but this does not

in the Bakerian lecture for 1807, that when potassium and sodium were burnt in oxygen gas, *the pure* alkalies were formed in a state of extreme dryness; and that 100 parts of potassium absorb about 18 parts of oxygen, and 100 parts of soda about 34 parts. Though, in the experiments from which these deductions were made, very small quantities only of the materials were employed; yet still, from frequent repetitions of the process, I hoped that they would approach to accuracy; and I am happy to find that this is the case; for the results differ very little in some experiments which I have made upon considerable portions of potassium and sodium, procured by chemical decomposition.

When potassium is burnt in trays of platina, in oxygen gas that has been dried by ignited potash, the absorption of oxygen is about $\frac{11}{20}$ of a cubical inch for every grain of the metal consumed; and when sodium is burnt in a similar manner, about a cubical inch is taken up for every grain.* The alkalies so formed, are only imperfectly fusible at a red heat; and do not, like the easily fusible alkalies, give indications of the presence of moisture.

M. D'Arcet has shown by some very well conducted inquiries, that potash and soda,† in their common state, contain a considerable proportion of water; and M. Berthollet concludes, that 100 parts of potash, that have been kept for some time in fusion, contain 13·89 parts of water, which is lost when the alkali enters into combination with muriatic acid; and the same sagacious observer, from some very minute experiments, infers, that muriate of potash, which has been ignited, contains in the 100 parts 66·66 potash, and 33·34 muriatic acid, a determination which differs very little from that of Bucholz.

To determine the relation of the dryness of the potash,

follow from their theory, unless they prove that water is given off in the operation, or combined with the borate of potash: the legitimate conclusion to be drawn from the processes, on their hypothesis, was, that they had made a hydruret of boracic acid.

* The quantities of gas given out by the operation of water, are in a similar ratio. See page 43 of the last Bakerian lecture [Phil. Mag. vol. xxxiii. p. 432.] and page 26 of this paper [p. 136 and 137 preceding.]

† *Annales de Chimie*, Nov. 1808, page 175.

formed from potassium, to that which has been considered as freed from the whole or the greatest part of its water, in muriate of potash, I made several experiments. I first attempted to convert a certain quantity of potassium into potash, upon the surface of liquid muriatic acid; but in this case the heat was so intense, and hydrogen holding potassium in solution was disengaged with so much rapidity, that there was a considerable loss of alkali; yet even under these circumstances, I obtained from ten parts of potassium 17·5 of dry muriate of potash. The most successful and the only mode which I employed, that can be entirely depended upon, was that of converting potassium into muriate of potash, in muriatic acid gas. I shall give the results of two experiments made in this manner: five grains of potassium inserted in a tray of platina, were made to act upon 19 cubical inches of muriatic acid gas, that had been exposed to muriate of lime; by the application of a gentle heat, the potassium took fire, and burnt with a beautiful red light;* and the whole mass appeared in igneous fusion; a little muriate of potash in the state of a white powder, sublimed and collected in the top of the vessel in which the experiment was made. Nearly 14 cubical inches of muriatic acid gas were absorbed, and about five of hydrogen were produced. The increase of weight of the tray was about 4·5 grains; and it did not lose any weight by being ignited.

The second experiment was conducted with still more attention to minuteness. Eight grains of potassium were employed; above 22 cubical inches of muriatic acid gas were consumed; the potassium burnt with the same brilliant phenomena as in the last experiment, and the increase of weight of the tray was $6\frac{1}{2}$ grains. The muriate of potash was kept for some minutes in fusion in the tray, till a white fume began to rise from it, but it did not lose the $\frac{1}{20}$ of a grain in weight. After the muriate of potash had been washed out of the tray, and it had been cleaned and dried, it was found to have lost about a third of a grain, which was platina in a metallic state, and that had alloyed

* As a retort exhausted of common air was used, the small quantity of residual common air may have been connected with this vividness of combustion.

with the potassium where it was in contact with the tray, during the combustion. There was no appearance of any water being separated in the process. A little muriate of potash sublimed; this was washed out of the retort, and obtained by evaporation: it did not equal $\frac{1}{8}$ of a grain.

Now if the data for calculation be taken from this last experiment, 8 grains of potassium will combine with 1·4 grains of oxygen, to form 9·4 grains of potash, and 6·6—1·4=5·2, the quantity of muriatic acid combined with the potash, which would give in the 100 parts in muriate of potash, 35·6 of acid, and 64·4 of potash; but 35·6 of muriatic acid, according to M. Berthollet's estimation, would demand 71.1 of alkali, in the state of dryness in which it exists in muriate of potash, and 71·1—64.4=6·7— so that the potash taken as a standard by M. Berthollet, contains at least 9 per cent. more water than that existing in the potash formed by the combustion of potassium in muriatic acid gas, which consequently may with much more propriety be regarded as the dry alkali.*

After these illustrations, I trust the former opinions which I ventured to bring forward, concerning the metals of the fixed alkalies, will be considered as accurate, and that potassium and sodium can with no more propriety be considered as *compounds*, than any of the common *metallic substances;* and that potash and soda, as formed by the combustion of the metals, are pure metallic oxides, in which no water is known to exist.

These conclusions must be considered as entirely independent of hypothetical opinions, concerning the existence of hydrogen in combustible bodies, as a common principle of inflammability, and of intimately *combined water*, as an essential constituent of acids, alkalies, and oxides: this part of the inquiry I shall reserve for the conclusion of the lecture, and I shall first consider the nature of the metal of ammonia, and the metals of the earths.

* Consequently M. Berthollet's fused potash must contain nearly 23 per cent. of water. From my own observations I am inclined to believe, that potash kept for some time in a red heat contains 16 or 17 per cent. of water, taking the potash formed by the combustion of potassium as the dry standard.

III. *Experiments on Nitrogen, Ammonia, and the Amalgam from Ammonia.*

One of the queries that I advanced, in attempting to reason upon the singular phenomena produced by the action of potassium upon ammonia, was, that nitrogen might possibly consist of oxygen and hydrogen, or that it might be composed from water.

I shall have to detail in this section a great number of laborious experiments, and minute and tedious processes, made with the hopes of solving this problem. My results have been for the most part negative; but I shall venture to state them fully, because I hope they will tend to elucidate some points of discussion, and may prevent other chemists from pursuing the same paths of inquiry, and which at first view do not appear unpromising.

The formation of nitrogen has been often asserted to take place in many processes, in which none of its known combinations were concerned. It is not necessary to enter into the discussion of the ideas entertained by the German chemists, on the origin of nitrogen, produced during the passage of water through red-hot tubes, or the speculations of Girtanner, founded on these and other erroneous data: the early discovery of Priestly on the passage of gases through red-hot tubes of earthen ware, the accurate researches of Berthollet, and the experiments of Bouillon La Grange, have afforded a complete solution of this problem.

One of the most striking cases, in which nitrogen has been supposed to appear without the presence of any other matter but water, which can be conceived to supply its elements, is in the decomposition and recomposition of water by electricity.* To ascertain if nitrogen could be generated in this manner, I had an apparatus made, by which a quantity of water could be acted upon by Voltaic electricity, so as to produce oxygen and hydrogen with great rapidity, and in which these gases could be detonated, without the exposure of the water to the atmos-

* See Dr. Pearson's elaborate experiments, on the decomposition of water by electrical explosions. Nicholson's Journal, 4to, vol. i. page 301.

phere; so that this fluid was in contact with platina, mercury, and glass only; and the wires for completing the Voltaic and common electrical circuit were hermetically inserted into the tube. 500 double plates of the Voltaic combination were used, in such activity that about the eighth of a cubical inch of the mixed gases, upon an average, was produced from 20 to 30 times in every day. The water used in this experiment was about a half a cubic inch; it had been carefully purged of air by the air pump, and by boiling, and had been introduced into the tube, and secured from the influence of the atmosphere whilst warm. After the first detonation of the oxygen and hydrogen, which together equalled about the eighth of a cubical inch, there was a residuum of about $\frac{1}{40}$ of the volume of the gases; after every detonation this residuum was found to increase, and when about 50 detonations had been made, it equalled rather more than $\frac{1}{4}$ of the volume of the water, *i. e.* $\frac{1}{8}$ of a cubical inch. It was examined by the test of nitrous gas; it contained no oxygen; six measures mixed with three measures of oxygen diminished to five; so that it consisted of 2·6 of hydrogen, and 3·4 of a gas having the characters of nitrogen.

This experiment seemed in favour of the idea of the production of nitrogen from pure water in these electrical processes; but though the platina wires were hermetically sealed into the tube, it occurred to me as possible that at the moment of the explosion by the electrical discharge, the sudden expansions and contractions might occasion some momentary communication with the external air through the aperture; and I resolved to make the experiments in a method by which the atmosphere was entirely excluded. This was easily done by plunging the whole of the apparatus, except the upper parts of the communicating wires, under oil, and carrying on the process as before. In this experiment the residuum did not seem to increase quite so fast as in the former one. It was carried on for nearly two months. After 340 explosions, the permanent gas equalled $\frac{24}{100}$ of a cubical inch. It was carefully examined: six measures of it, detonated with three measures of oxygen, diminished to rather less than one measure; a result which seems to show, that nitrogen is not formed during the electrical decomposition and recomposition of water, and that the residual gas is hydro-

gen. That the hydrogen is in excess, may be easily referred to a slight oxidation of the platina.

The refined experiments of Mr. Cavendish on the deflagration of mixtures of oxygen, hydrogen, and nitrogen, lead directly to the conclusion, that the nitrous acid sometimes generated in experiments on the production of water, owes its origin to nitrogen, mixed with the oxygen and hydrogen, and is never produced from those two gases alone. In the Bakerian lecture for 1806, I have stated several facts, which seem to show that the nitrous acid, which appears in many processes of the Voltaic electrization of water, cannot be formed, unless *nitrogen* be present.

Though in these experiments I endeavoured to guard with great care against all causes of mistake, and though I do not well see how I could fall into an error, yet I find that the assertion, that both acids and alkalies may be produced from pure water, has again been repeated.* The energy with which the large Voltaic apparatus, recently constructed in the Royal Institution, acts upon water, enabled me to put this question to a more decided test than was before in my power. I had formerly found in an experiment, in which pure water was electrified in two gold cones in hydrogen gas, that no nitrous acid nor alkali was formed. It might be said, that in this case the presence of hydrogen dissolved in the water, would prevent nitrous acid from appearing; I therefore made two series of experiments, one in a jar filled with oxygen gas, and the other in an apparatus in which glass, water, mercury, and wires of platina only, were present.

In the first series 1000 double plates were used, the two cones were of platina, and contained about $\frac{1}{12}$ of a cubical inch each, and filaments of asbestus were employed, to connect them together. In these trials, when the batteries were in full action, the heat was so great, and the gases were disengaged with so much rapidity, that more than half the water was lost in the course of a few minutes. By using a weaker charge, the process was carried on for some hours, and in some cases for from two to three days. In no instance, in which slowly distilled

* Nicholson's Journal, August 1809, p. 258.

water was employed, and in which the receiver was filled with pure oxygen from oxymuriate of potash, was any acid or alkali exhibited in the cones; even when nitrogen was present, the indications of the production of acid and alkaline matter were very feeble; though, if the asbestus was touched with unwashed hands, or the smallest particle of neutro-saline matter introduced, there was an immediate separation of acid and alkali, at the points of contact of the asbestus with the platina, which could be made evident by the usual tests.

In the second series of experiments, the oxygen and hydrogen produced from the water were collected under mercury, and the two portions of water communicated directly with each other. In several trials made in this way, with a combination of 500 plates, and continued for some days, it was always found that fixed alkali separated in the glass negatively electrified; and a minute quantity of acid, which could barely be made evident by litmus, in the glass positively electrified. This acid rendered cloudy nitrate of silver. Whether its presence was owing to impurities which might rise in distillation with the mercury, or to muriatic acid existing in the glass, I cannot say; but as common salt perfectly dry is not decomposed by silex, it seems very likely that muriatic acid in its arid state may exist in combination in glass.

I tried several experiments on the ignition and fusion of platina by Voltaic electricity, in mixtures of the vapour of water and oxygen gas. I thought it possible, if water could be combined with *more oxygen*, that this heat, the most intense we are acquainted with, might produce the effect. When the oxygen was mixed with nitrogen, nitrous acid was formed; but when it consisted of the last portions from oxymuriate of potash, there was not the slightest indication of such a result.

Water in vapour was passed through oxide of manganese, made redhot in a glazed porcelain tube, the bore of which was nearly an inch in diameter; in this case a solution of nitrous acid, sufficiently strong to be disagreeably sour to the taste, and which readily dissolved copper, was formed.

This experiment was repeated several times, and, when the diameter of the tube was large, with precisely the same results. When red oxide of lead was used instead of oxide of manga-

nese, no acid was however generated; but upon this substance a single trial only was made, and that in a small tube, so that no conclusion can with propriety be drawn from this failure.

I stated in the last Bakerian lecture, that in attempting to produce ammonia from a mixture of charcoal and pearlash, that had been ignited by the action of water, in the manner stated by Dr. Woodhouse, I failed in the trial in which the mixture was cooled in contact with hydrogen. I have since made a number of similar experiments. In general, when the mixture had not been exposed to air, there was little or no indication of the production of the volatile alkali; but the result was not so constant as to be entirely satisfactory; and the same circumstances could not be uniformly obtained in this simple form of the experiment. I had an apparatus made, in which the phenomena of the process could be more rigorously examined. Pure potash and charcoal, in the proportion of one to four in weight, were ignited in the middle of a tube of iron, furnished with a system of stopcocks, and connected with a pneumatic apparatus, in such a manner that the mixture could be cooled in contact with the gas produced during the operation; and that water exhausted of air could be made to act upon the cooled mixture, and afterwards distilled from it: figures of this apparatus, and an account of the manner in which it was used, are annexed to this paper. In this place I shall state merely the general results of the operations, which were carried on for nearly two months, a variety of precautions being used to prevent the interference of nitrogen from the atmosphere.

In all cases in which the water was brought in contact with the mixture of charcoal and potash, when it was perfectly cool, and afterwards distilled from it by a low heat, it was found to hold in solution small quantities of ammonia; when the operation was repeated upon the same mixture, ignited a second time, the proportion diminished; in a third operation it was sensible, but in the fourth barely perceptible. The same mixture, however, by the addition of a new quantity of potash, again gained the power of producing ammonia in two or three successive operations; and when any mixture had ceased to give ammonia, the power was not restored by cooling it in contact with air.

Ammonia was produced in a case in which more than 200 cubical inches of gas had passed over from the action of water upon a mixture, and when the last portions only were preserved in contact with it during the cooling. In a comparative trial it was however found, that considerably more ammonia was produced, when a mixture was cooled in contact with the atmosphere, than when it was cooled in contact with the gas developed in the operation.

I shall not attempt to draw any conclusions from these processes. It would appear from some experiments of M. Berthollet, that nitrogen adheres very strongly to charcoal.* The circumstances that the ammonia ceases to be produced after a certain number of operations, and that the quantity is much greater when free nitrogen is present, are perhaps against the idea that nitrogen is composed in the process. But till the weights of the substances concerned and produced in these operations are compared, no correct decision on the question can be made.

The experiments of Dr. Priestly upon the production of nitrogen, during the freezing of water, induced that philosopher to conceive, either that water was capable of being converted into nitrogen, or that it contained much more nitrogen than is usually suspected.

I have made some repetitions of his processes. A quantity of water, (about a cubical inch and a quarter,) that had been produced from snow, boiled and inverted over mercury whilst hot, was converted into ice, and thawed in 16 successive operations; gas was produced, but after the first three or four times of freezing there was no notable increase of the volume. At the end of the experiment, about $\frac{1}{50}$ of a cubical inch was obtained, which proved to be common air.

About four cubical inches of water from melted snow were converted into ice and thawed, four successive times, in a conical vessel of wrought iron. At the end of the fourth process, the volume of gas equalled about $\frac{1}{20}$ of the volume of the water. It proved to contain about $\frac{1}{10}$ oxygen, $\frac{3}{10}$ hydrogen, and $\frac{6}{10}$ nitrogen.

* *Mem. d'Arcueil*, tom. ii. page 485.

Mr. Kirwan observed the fact, that when nitrous gas and sulphuretted hydrogen are kept in contact for some time, there is a great diminution of volume, and that the nitrous gas becomes converted into nitrous oxide, and that sulphur is deposited, which has an ammoniacal smell. I repeated this experiment several times in 1800 with similar results; and I found, that the diminution of the volume of the gases, when they were mixed in equal proportions, was to rather less than $\frac{1}{4}$, which seemed to be nitrous oxide.

In reasoning upon this phenomenon, I saw grounds for a minute investigation of it. Sulphuretted hydrogen, as appears from experiments which I have stated on a former occasion, and from some that I shall detail towards the conclusion of this lecture, contains a volume of hydrogen equal to its own. But one of hydrogen demands half its volume of oxygen to convert it into water, and nitrous gas consists of about half a part in volume of oxygen; so that, supposing the whole of the hydrogen employed in absorbing oxygen from nitrous gas, nitrogen alone ought to be formed, and not nitrous oxide. Or, if the whole of the gas is nitrous oxide, this should contain all the nitrogen of the nitrous gas, leaving none to be supplied to the ammonia. I mixed together five cubical inches of nitrous gas, and five of sulphuretted hydrogen over mercury, the barometer being at 29·5^{in}., thermometer at 51° Fahrenheit; twelve hours had elapsed before any change was perceived; there was then a whitish precipitate formed, and a deep yellow liquid began to appear in drops, on the inside of the jar, and the volume of the gases quickly diminished; after two days the diminution ceased, and the volume became stationary; the barometer was at 30·45^{in}., and thermometer 52° Fahrenheit; when it equalled 2·3. The gas proved to be about $\frac{3}{4}$ nitrous oxide, and the remaining fourth was inflammable. An experiment was made expressly to determine the nature of the deep yellow liquid in the jar. It proved to be of the same kind as Boyle's fuming liquor, the hydrosulphuret of ammonia, but with sulphur in great excess.

In this experiment there was evidently no formation of nitrogen, and these complicated changes ended in the production of two new compounds; nitrogen and hydrogen; oxygen and sul-

phur combining to form one; and a part of the nitrogen and oxygen, becoming more condensed, to form another.

Having stated the results of the investigation on the production of nitrous acid and of ammonia, in various processes of chemistry, I shall notice some attempts that I made to decompound nitrogen, by agents which I conceived might act at the same time on oxygen, and on the basis of nitrogen. Potassium, as I have before stated, sublimes in nitrogen, without altering it, or being itself changed: but I thought it possible, that the case might be different, if this powerful agent were made to act upon nitrogen, assisted by the intense heat and decomposing energy of Voltaic electricity.

I had an apparatus made, by which the Voltaic circuit could be completed in nitrogen gas, confined by mercury, by means of potassium and platina. The potassium, in the quantity of about two or three grains, was placed in a cup of platina, and by contact with a wire of platina it could be fused and sublimed in the gas. The quantity of nitrogen was usually about a cubical inch. The battery employed was always in full action for these experiments, and consisted of one thousand double plates. The phenomena were very brilliant: as soon as the contact with the potassium was made there was always a bright light, so intense as to be painful to the eye; the platina became white hot; the potassium rose in vapour; and by increasing the distance of the cup from the wire, the electricity passed through the vapour of the potassium, producing a most brilliant flame, of from half an inch to an inch and a quarter in length; and the vapour seemed to combine with the platina, which was thrown off in small globules in a state of fusion, producing an appearance similar to that produced by the combustion of iron in oxygen gas.

In all trials of this kind, hydrogen was produced; and in some of them there was a loss of nitrogen. This at first seemed to lead to the inference that nitrogen is decompounded in the process; but I found that, in proportion as the potassium was introduced more free from a *crust of potash*, which would furnish water and consequently hydrogen in the experiment, so in proportion was there less of this gas evolved; and in a case in

which the greatest precautions were taken; the quantity did not equal $\frac{1}{1}$ of the volume of gas, and there was no sensible quantity of nitrogen lost.

The largest proportion of nitrogen which disappeared in any experiment, was $\frac{1}{11}$ of the quantity used; but in this case the crust of potash was considerable, and a volume of hydrogen, nearly equal to $\frac{1}{4}$ of the nitrogen, was produced. It cannot be said, that the nitrogen is *not* decomposed in this operation; but it seems much more likely that the slight loss is owing to its combination with nascent hydrogen, and its being separated with the potassium in the form of the gray pyrophoric sublimate, which I have found is always produced when potassium is electrized and converted into vapour in ammonia.

The phosphuret of lime in its common state is a conductor of electricity; and when it was made the medium of communication between the wires of the great battery, it burnt with a most intense light. It was ignited to whiteness in nitrogen gas; a little phosphuretted hydrogen was given off from it, but the nitrogen was not altered; the apparatus was similar to that used for the potassium.

As almost all compounds known to contain hydrogen are readily decomposed by oxymuriatic acid gas, a mixture of nitrogen and oxymuriatic acid gas was passed through a porcelain tube heated to whiteness; the products were received in a pneumatic apparatus over water, there was a small loss of nitrogen; but the greatest part came over densely clouded; and as nitro-muriatic acid was found dissolved in the water, no conclusions concerning the decomposition of nitrogen can be drawn from the process.

The general tenor of these inquiries cannot be considered as strengthening, in any considerable degree, the suspicion which I formed of the decomposition of nitrogen, by the distillation of the olive-coloured substance from potassium and ammonia, in tubes of iron.

In reasoning closely upon the phenomena in this operation, it appears to me indeed possible to account for the loss of nitrogen, without assuming that it has been converted into new matter. Though the iron tubes which I used were carefully cleaned; yet still it was not unlikely that a small quantity of

oxide might adhere to the welded parts; the oxygen of which, in the beginning of the process of distillation, might form water with hydrogen, given off from the fusible substance; which, being condensed in the upper part of the tube, would be again brought into action towards the close of the operation, occasioning the formation, and possibly the absorption of some ammonia, and consequently a loss of nitrogen, and the production of an increased proportion of hydrogen. I have made one experiment, with the hopes of deciding this question, in an iron tube used immediately after the whole internal surface had been cleaned by the borer; six grains of potassium were used in a tray of iron, nearly thirteen cubical inches of ammonia were absorbed, and about six of hydrogen produced. Thirteen cubical inches of gas were evolved in the first operation; which consisted of nearly one cubical inch of ammonia, four of nitrogen, and eight of hydrogen. The portion of gas given off in the second operation equalled 3·6 cubical inches; which consisted of 2·5 hydrogen, and 1·1 nitrogen. The potassium produced in the operation was sufficient to generate 3·1 cubical inches of hydrogen.

As the iron in these experiments had been heated to intense whiteness, and must have been very soft; it was not impossible, considering the recent experiments of M. Hassenfratz,* that the loss of so large a portion of potassium might depend upon an intimate union of that body with iron, and its penetration into the substance of the tube. This idea is countenanced by another experiment of the same kind, in which the heat was raised to whiteness, and the barrel cut into pieces when cool: on examining the lower part of it, I found in it a very thin film of potash; but which, I conceive, could scarcely equal a grain in weight. The pieces of the barrel were introduced under a jar inverted in water; at the end of two days nearly 2·3 cubical inches of hydrogen were found to be generated.

In the experiments detailed in page 53 of the last volume of the Transactions,† a loss of nitrogen, and a production of hydrogen, was perceived in a case in which the residuum from a

* *Journal des Mines, Avril* 1808, p. 275.

† Phil. Mag. vol. xxxiii. page 8.

portion of fusible substance, which had been exposed to a low red heat, was distilled in a tube of platina; but in this case the residuum had been covered by *naphtha*, and it is possible that ammonia might have been regenerated by hydrogen from the naphtha, and absorbed by that fluid; and a part of the hydrogen might likewise proceed from the decomposition of the naphtha: and in several experiments in which I have burnt the entire fusible substance, I have found no loss of nitrogen.

Even the considerable excess of hydrogen, and deficiency of nitrogen, in the processes in which the fusible substance is distilled with a new quantity of potassium, page 451,* it is possible to refer to the larger quantity of moisture, which must be absorbed by the fusible substance from the air, during the time occupied in attaching the potassium to the tray, and likewise from the moisture adhering to the crust of potash, which always forms upon the potassium, during its exposure to air.

These objections are the strongest that occur to me, against the mode of explaining the phenomena by supposing nitrogen decomposed in the operation; but they cannot be considered as decisive on this complicated and obscure question, and the opposite view may be easily defended.

Though I have already laid before the Society a number of experiments upon the decomposition of ammonia, yet I shall not hesitate to detail some further operations which have been conducted according to new views of the subject.

I concluded from the loss of weight taking place in the electrical analysis of ammonia, that water or oxygen was probably separated in this operation; but I was aware that objections might be made to this mode of accounting for the phenomenon.

The experiment of producing an amalgam from ammonia, which regenerated volatile alkali, apparently by oxidation, confirmed the notion of the existence of oxygen in this substance; at the same time it led to the suspicion, that of the two gases separated by electricity, one, or perhaps both, might contain metallic matter united to oxygen: and the results of the distillation of the fusible substance, from potassium and ammonia, notwithstanding the objections I have made, can perhaps be explained on such a supposition.

* Phil. Mag. vol. xxxiv. page 339.

I have made a number of experiments upon the decomposition of considerable quantities of ammonia, both by Voltaic and common electricity; and I have used an apparatus (of which a figure is attached to this paper) in which nothing was present but the gas, the metals for conveying the electricity, and glass. The ammonia was introduced by a stopcock which was cleared of common air, into a globe that was exhausted, after being filled two or three times with ammonia: the gas that was used was absolutely pure, the decomposition was performed without any possibility of change in the volume of the elastic matter, and the apparatus was such, that the gas could be exposed to a *freezing mixture*, and the whole weighed before and after the experiment.

The object in keeping the volume the same during the decomposition, was to produce the condensation of any aqueous vapour, which if formed in small quantity in the operation, (on the theory of the mechanical diffusion of vapour in gases,) might in the common case of decomposition, under the usual pressure, be in quantity nearly twice as much in the hydrogen and nitrogen, as in the ammonia.

In all instances it was found, that there was no loss of weight of the apparatus, nor was there any deposition of moisture, during or after the electrization; but the wires were uniformly tarnished; and in an experiment in which surfaces of brass were used, a small quantity of olive-coloured matter formed on the metal; but though in this case nearly eight cubical inches of ammonia were decomposed, the weight of the oxidated matter was so minute as to be scarcely sensible. By the use of a freezing mixture of muriate of lime and ice, which diminished the temperature to —15°, there was a very feeble indication given of the addition of hydrometrical moisture.

In these experiments the increase of the gas was uniformly (within a range of five parts) from 100 to 185, and the hydrogen was to the nitrogen in the average proportions of from 73·74 to 27·26; the proper corrections being made, and the precautions before referred to being taken.*

Assuming the common estimations of the specific gravity of

* Philosophical Transactions 1809, page 459. M. Bertholett, jun. in the second volume of the Memoirs of Arcueil, has given a paper on the decom-

ammonia, of hydrogen, and nitrogen, the conclusions which I have advanced in the Bakerian lecture for 1807 would be supported by these new experiments; but as the moisture and oxygen visibly separated cannot be conceived to be as much as $\frac{1}{11}$ or $\frac{1}{12}$ of the weight of the ammonia, I resolved to investigate, more precisely than I had reason to think had been hitherto done, the specific gravities of the gases concerned in their dry state; and the very delicate balance belonging to the Royal Institution placed the means of doing this in my power.

Nitrogen, hydrogen, and ammonia, were dried by a long continued exposure to potash, and were very carefully weighed. Their relative specific gravities proved to be at 30·5$^{in.}$ barometer, 51° Fahrenheit's thermometer.

For nitrogen, the 100 cubical inches.....29·8 grains.
For hydrogen, ditto..... 2·27
For ammonia.....18·4

Now, if these data be calculated upon, it will be found, that in the decomposition of 100 of ammonia, taking even the largest proportions of gases evolved, there is a loss of $\frac{1}{18}$;* and if the smallest proportion be taken, the loss will be nearly $\frac{1}{12}$.

position of ammonia, and he enters into an examination of my idea of the oxygen separated in the electrical decomposition of ammonia, which he supposes I rate at 20 per cent., and at the same time he confutes some experiments which he is pleased to attribute to me, of the combustion of charcoal and iron in ammonia. His arguments and his facts upon these points appear to me perfectly conclusive; but as I never formed such an opinion, as that 20 of oxygen were separated in the experiment, and never imagined such results as the combustion of iron and charcoal in ammonia, and never published any thing which could receive such an interpretation, I shall not enter into any criticism on this part of his paper. The experiments of this ingenious chemist on the direct decomposition of ammonia seem to have been conducted with much care, except as to the circumstance of his not boiling the quicksilver; which I conceive has occasioned him to overrate the increase of volume. At all events a loss of weight is more to be expected than an increase of weight, in all very refined experiments of this kind. It is possible that the volume may be exactly doubled, and that the nitrogen may be to the hydrogen as one to three; but neither the numerous experiments of Dr. Henry, nor those that I have tried, establish this; it is one of the hypothetical inferences that may be made, but it cannot be regarded as an absolute fact.

* 100 of ammonia at the rate of 185, will give 136·9 of hydrogen, weighing 3·1 grains, and 48·1 of nitrogen, weighing 14·33 grains; but 18·4—17·4=1, and at the rate of 180, 133 of hydrogen weighing 3·01 and 47 of nitrogen weighing 14, and 18·4—17=1·4.

These results and calculations agree with those that I have before given, and with those of Dr. Henry.

The lately discovered facts in chemistry, concerning the important modifications which bodies may undergo by very slight additions or subtractions of new matter, ought to render us cautious in deciding upon the nature of the process of the electrical decomposition of ammonia.

It is *possible*, that the minute quantity of oxygen which appears to be separated is not accidental, but a result of the decomposition; and if hydrogen and nitrogen be both oxides of the same basis, the possibility of the production of different proportions of water, in different operations, might account for the variations observed in some cases in their relative proportions; but on the whole, the idea that ammonia is decomposed into hydrogen and nitrogen alone, by electricity, and that the loss of weight is no more than is to be expected in processes of so delicate a kind, is, in my opinion, the most defensible view of the subject.

But if *ammonia* be capable of decomposition into nitrogen and hydrogen, what, it will be asked, is the nature of the matter existing in the amalgam of ammonia? what is the metallic basis of the volatile alkali? These are questions intimately connected with the whole of the arrangements of chemistry; and they are questions, which, as our instruments of experiment now exist, it will not, I fear, be easy to solve.

I have stated in my former communication on the amalgam from ammonia, that, under all the common circumstances of its production, it seems to preserve a quantity of water adhering to it, which may be conceived to be sufficient to oxidate the metal, and to reproduce the ammonia.

I have tried various devices with the hopes of being able to form it from ammonia in a dry state, but without success. Neither the amalgams of potassium, sodium, or barium, produce it in ammoniacal gas; and when they are heated with muriate of ammonia, unless the salt is moist, there is no metallization of the alkali.

I have acted upon ammonia by different metallic amalgams negatively electrified, such as the amalgams of gold and silver, the amalgam of zinc, and the liquid amalgam of bismuth and

lead; but in all these cases the effect was less distinct than when pure mercury was used.

By exposing the mercury to a cold of —20° Fahrenheit, in a close tube, I have succeeded in obtaining an amalgam in a much more solid state; yet this decomposed nearly as rapidly as the common amalgam, but it gave off much more gaseous matter; and in one instance I obtained a quantity which was nearly equal to six times its volume.

The amalgam which I have reason to believe can be made most free from *adhering moisture*, is that of potassium, mercury, and ammonium in a solid state. This, as I have mentioned in my former communication, decomposes very slowly, even in contact with water, and, when it has been carefully wiped with bibulous paper, bears a considerable heat without alteration. I have lately made several new attempts to distil the ammonium from it, but without success. When it is strongly heated in a green glass tube filled with hydrogen gas, there is always a partial regeneration of ammonia; but with this ammonia there is from $\frac{5}{10}$ to $\frac{6}{10}$ of hydrogen produced.

As it does not seem possible to obtain an amalgam in an uniform state, as to adhering moisture, it is not easy to say what would be the exact ratio between the hydrogen and ammonia produced, if no more water was present than would be decomposed in oxidating the basis. But in the most refined experiments which I have been able to make, this ratio is that of one to two; and in no instance in which proper precautions are taken, is it less; but under common circumstances often more. If this result is taken as accurate, then it would follow, that ammonia (supposing it to be an oxide) must contain about 48 per cent. of oxygen, which, as will be hereafter seen, will agree with the relations of the attractions of this alkali for acids, to those of other salifiable bases.*

* Even in common air, the amalgam evolves hydrogen and ammonia, nearly in these proportions, and in one experiment which I lately tried, there seemed to be no absorption of oxygen from the atmosphere. This circumstance appears to me in favour of the antiphlogistic view of the metallization of the volatile alkali; for if the hydrogen be supposed to be given off from the mercury, and not to arise from the decomposition of water adhering to the amalgam, it might be conceived, that being in the nascent state, it would rap-

If hydrogen be supposed to be a simple body, and nitrogen an oxide, then, on the hypothesis above stated, nitrogen would consist of nearly 48 of oxygen, and 34 of basis; but if the opinion be adopted, that hydrogen and nitrogen are both oxides of the same metal, then the quantity of oxygen in nitrogen must be supposed less.

These views are the most obvious that can be formed, on the antiphlogistic hypothesis, of the nature of metallic substances; but if the facts concerning ammonia were to be reasoned upon, independently of the other general phenomena of chemical science, they perhaps might be more easily explained on the notion of nitrogen being a basis, which became alkaline by combining with one portion of hydrogen, and metallic, by combining with a greater proportion.

The solution of the question concerning the quantity of matter added to the mercury in the formation of the amalgam, depends upon this discussion; for, if the phlogistic view of the subject be adopted, the amalgam must be supposed to contain nearly twice as much matter as it is conceived to contain on the hypothesis of deoxygenation. In the last Bakerian lecture, I have rated the proportion at $\frac{1}{12000}$; but this is the least quantity that can be assumed, the mercury being supposed to give off only one and a half its volume of ammonia. If the proportion stated in page 56 [page 160 preceding] be taken as the basis of calculation, which is the maximum that I have obtained, the amalgam would contain about $\frac{1}{1500}$ of new matter, on the antiphlogistic view, and about $\frac{1}{900}$ on the phlogistic view.

I shall have occasion to recur to, and to discuss more fully, these ideas, and I shall conclude this section by stating, that though the researches on the decomposition and composition of nitrogen, which have occupied so large a space in the foregoing pages, have been negative, as to the primary object, yet

idly absorb oxygen. In my first experiments upon the amalgam, finding that common air, to which it had been exposed, gave less diminution with nitrous gas than before, I concluded naturally, that oxygen had been absorbed; but this difference might have arisen, partly at least, from the mixture of hydrogen. Whether in any case the amalgam absorbs oxygen gas, is a question for further investigation.

they may not possibly be devoid of useful applications. It does not seem improbable, that th passage of steam over hot manganese may be applied to the manufacture of nitrous acid. And there is reason to believe that the ignition of charcoal and potash, and their exposure to water, may be advantageously applied to the production of volatile alkali, in countries where fuel is cheap.

IV. *On the Metals of Earths.*

I have tried a number of experiments with the hopes of gaining the same distinct evidences of the decomposition of the common earths, as those afforded by the electro-chemical processes on the alkalies, and the alkaline earths.

I find that when iron wire ignited to whiteness, by the power of 1000 double plates, is negatively electrified and fused in contact with either silex, alumine or glucine, slightly moistened and placed in hydrogen gas; the iron becomes brittle and whiter, and affords by solution in acids, an earth of the same kind as that which has been employed in the experiment.

I have passed potassium in vapor through each of these earths, heated to whiteness in a platina tube: the results were remarkable, and perhaps not unworthy of being fully detailed.

When silex was employed, being in the proportion of about ten grains to four of potassium, no gas was evolved, except the common air of the tube mingled with a little inflammable gas, not more than might be referred to the moisture in the crust of alkali, formed upon the potassium. The potassium* was entirely destroyed; and glass with excess of alkali was formed in the lower part of the tube; when this glass was powdered, it exhibited dark specks, having a dull metallic character not unlike that of the protoxide of iron. When the mixture was thrown into water, there was only a very slight effervescence;

* The results of this experiment are opposed to the idea that potassium is a compound of hydrogen and potash or its basis; for if so, it might be expected that the hydrogen would be disengaged by the attraction of the alkali for silex. In my first experiments on this combination, I operated in an apparatus connected with water, and I found that the potassium produced as much hydrogen as if it had been made to act upon water; in this case the metal had rapidly decomposed the vapor of the water, which must have been constantly supplied.

but on the addition of muriatic acid to the water, globules of gas were slowly liberated, and the effect continued for nearly an hour, so that there is great reason to believe, that the silex had been either entirely or partially deoxygenated, and was slowly reproduced by the action of the water, assisted by the slight attraction of the acid for the earth.

When the potassium was in the quantity of six grains, and the silex of four grains, a part of the result inflamed spontaneously as it was taken out of the tube, though the tube was quite cool, and left, as the result of its combustion, alkali and silex. The part which did not inflame, was similar in character to the matter which has been just described, it did not act upon water, but effervesced with muriatic acid.

Potassium in acting upon alumine and glucine, produced more hydrogen than could be ascribed to the moisture present in the crust of potash; from which it seems probable that even after ignition, water adheres to these earths.

The results of the action of the potassium were pyrophoric substances of a dark gray colour, which burnt, throwing off brilliant sparks,* and leaving behind alkali and earth, and which hissed violently when thrown upon water, decomposing it with great violence. I examined the products in two experiments, one on alumine, and one on glucine, in which naphtha was introduced into the platina tube, to prevent combustion; the masses were very friable, and presented small metallic particles, which were as soft as potassium, but so small that they could not be separated, so as to be more minutely examined; they melted in boiling naphtha. Either a part of the potassium must have been employed in decomposing the earths in these experiments, or it had entered into combination with them; which is unlikely, and contrary to analogy, and opposed by some experiments which will be immediately related.

Supposing the metals of the earths to be produced in experiments of this kind, there was great reason to expect that they might be alloyed with the common metals, as well as with potassium. Mercury was the only substance which it was safe

* The pyrophorus from alum, which I have supposed in the last Bakerian lecture to be a compound of potassium, sulphur, and charcoal, probably contains this substance likewise.

to try in the tube of platina. In all cases in which the potassium was in excess, I obtained amalgams by introducing mercury, whilst the tube was hot; but the alkaline metal gave the characters to the amalgam, and though in the case of glucine and alumine, a white matter separated during the action of very weak muriatic acid upon the amalgam, yet I could not be entirely satisfied that there was any of the metals of these earths in triple combination.

Mixtures of the earths with potassium, intensely ignited in contact with iron filings, and covered with iron filings in a clay crucible, gave much more distinct results. Whether silex, alumine, or glucine was used, there was always a fused mass in the centre of the crucible; and this mass had perfectly metallic characters. It was in all cases much whiter and harder than iron. In the instance in which silex was used, it broke under the hammer, and exhibited a crystalline texture. The alloys from alumine and glucine were imperfectly malleable. Each afforded, by solution in acids, evaporation, and treatment with reagents, oxide of iron, alkali, and notable quantities of the earth employed in the experiment.

Though I could not procure decided evidences of the production of an amalgam, from the metals of the common earths, yet I succeeded perfectly by the same method of operating, in making amalgams of the alkaline earths.

By passing potassium through lime and magnesia, and then introducing mercury, I obtained solid amalgams, which consisted of potassium, the metal of the earth employed, and mercury.

The amalgam from magnesia was easily deprived of its potassium by the action of water. It then appeared as a solid white metallic mass, which by exposure to the air became covered with a dry white powder, and which when acted upon by weak muriatic acid, gave off hydrogen gas in considerable quantities, and produced a solution of magnesia.

By operations performed in this manner, there is good reason to believe, it will be possible to procure quantities of the metals of the alkaline earths, sufficient for determining their nature and agencies, and the quantities of oxygen which they absorb; and by the solution of the alloys containing the metals

of the common earths, it seems probable, that the proportions of metallic matter in these bodies may likewise be ascertained.

On an hypothesis which I have before brought before the Society, namely, that the power of chemical attraction and electrical action may be different exhibitions of the same property of matter, and that oxygen and inflammable bodies are in relations of attraction which correspond to the function of being negative and positive respectively, it would follow that the attractions of acids for salifiable bases would be inversely as the quantity of oxygen that they contain; and supposing the power of attraction to be measured by the quantity of basis which an acid dissolves, it would be easy to infer the quantities of oxygen and metallic matter from the quantities of acid and of basis in a neutral salt. On this idea I had early in 1808 concluded that barytes must contain least oxygen of all the earths, and that the order as to the quantity of inflammable matter must be strontites, potash, soda, lime, and so on; and that silex must contain the largest quantity of oxygen of all.

If the most accurate analyses be taken, barytes may be conceived to contain about 90·5* of metal per cent. strontites 86,† lime 73·5,* magnesia 66.‡

The same proportion would follow from an application of Mr. Dalton's ingenious supposition,§ that the proportion of

* Mr. James Thomson, Nicholson's Journal, 1809, p. 175, and Berthier.

† Mr. Clayfield. Thomson's Chemistry, vol. ii. p. 626, 629.

‡ Murray's Chemistry, vol. iii. p. 616.

§ The principle that I have stated of the affinity of an acid for a salifiable basis being inversely as the quantity of oxygen contained by the basis, though gained from the comparison of the electrical relations of the earths, with their chemical affinities, in its numerical applications, must be considered merely as a consequence of Mr. Dalton's law of general proportions. Mr. Dalton had indeed, in the spring of 1808, communicated to me a series of proportions for the alkalies and alkaline earths; which, in the case of the alkalies, were not very remote from what I had ascertained by direct experiments. M. Gay Lussac's principle, that the quantity of acid in metallic salts is directly as the quantity of oxygen, might (as far as it is correct) be inferred from Mr. Dalton's law, though this ingenious chemist states that he was led to it by different considerations. According to Mr. Dalton, there is a proportion of oxygen, the same in all protoxides, and there is a proportion of acid, the same in all neutral salts; and new proportions of oxygen and of acid are

oxygen is the same in all protoxides, and that the quantity of acid is the same in all neutral salts, *i. e.* that every neutral salt is composed of one particle of metal, one of oxygen, and one of acid.

We are in possession of no accurate experiments on the quantity of acids required to dissolve alumine, glucine, and silex; but according to Richter's estimation of the composition* of phospate of alumine, alumine would appear to contain about 56 per cent. of metallic matter.

M. Berzelius,† in a letter which I received from him a few months ago, states, that in making an analysis of cast iron, he found that it contained the metal of silex, and that this metal in being oxidated took up nearly half its weight of oxygen.

If the composition of ammonia be calculated upon, according to the principle above stated, it ought to consist of 53 of metallic matter, and about 47‡ of oxygen, which agrees very nearly with the quantity of hydrogen and ammonia produced from the amalgam.

Though the early chemists considered the earths and the metallic oxides as belonging to the same class of bodies, and the earths as calces which they had not found the means of

always multiples of these proportions. So that if a protoxide in becoming a deutoxide takes up more acid, it will be at least double the quantity, and in these cases the oxygen will be strictly as the acid. Mr. Dalton's law even provides for cases to which M. Gay Lussac's will not apply, a deutoxide may combine with a single quantity of acid, or a protoxide with a double quantity. Thus in the insoluble oxysulphat of iron perfectly formed, (as some experiments which I have lately made seem to show,) there is probably only a single proportion of acid; and in the super-tartrite of potash there is only a single quantity of oxygen, and a double quantity of acid. Whether Mr. Dalton's law will apply to *all cases*, is a question which I shall not in this place attempt to discuss.

* Thomson's Chemistry, vol. ii. p. 581.

† In the same communication this able chemist informed me, that he had succeeded in decomposing the earths, by igniting them strongly with iron and charcoal.

‡ I take the proportions of the volumes from the very curious paper of M. Gay Lussac, on the combinations of gaseous bodies, *Mem. d'Arcueil*, tom. ii. page 213, and the weights from my own estimation, according to which 100 cubic inches of muriatic acid gas weigh 39 grains, at the mean temperature and pressure, which is very nearly the same as the weight given by MM. Gay Lussac and Thenard.

combining with phlogiston, and though Lavoisier insisted upon this analogy with his usual sagacity, yet still the alkalies, earths, and oxides have been generally considered as separate natural orders. The earths, it has been said, are not precipitated by the triple prussiates, or by the solutions of galls;* and the alkalies and alkaline earths are both distinguished by their solubility in water; but if such characters be admitted as grounds of distinct classification, the common metals must be arranged under many different divisions; and the more the subject is inquired into, the more distinct will the general relations of all metallic substances appear. The alkalies and alkaline earths combine with prussic acid, and form compounds of different degrees of solubility; and solutions of barytes (as has been shown by Dr. Henry and M. Guyton) precipitate the triple prussiate of potash; the power of combination is general, but the compounds formed are soluble in different degrees in water. The case is analogous with solutions of galls; these, as I have mentioned in a paper published in the Philosophical Transactions for 1805, are precipitated by almost all neutro-saline solutions; and they form compounds more or less soluble in water, more or less coloured, and differently coloured with all salifiable bases. It is needless to dwell upon the combinations of the alkalies and earths, with oils, to form soaps; and of the earthy soaps, some are equally insoluble with the metallic soaps. The oxide of tin, and other oxides abounding in oxygen, approach very near in their general characters to zircon, silex, and alumine; and in habits of amalgamation, and of alloy, how near do the metals of the alkalies approach to the lightest class of oxidable metals!

It will be unnecessary, I trust, to pursue these analogies any further, and I shall conclude this section by a few remarks on the alloys of the metals of the common earths.

It is probable that these alloys may be formed in many metallurgical operations, and that small quantities of them may influence materially the properties of the compound in which they exist.

In the conversion of cast into malleable iron, by the process of blooming, a considerable quantity of glass separates, which,

* *Klaproth, Annales de Chimie*, tome x. p. 277.

as far as I have been able to determine, from a coarse examination, is principally silex, alumine, and lime, vitrified with oxide of iron.

Cast iron from a particular spot will make only cold-short iron; whilst, from another spot, it will make hot-short; but, by a combination of the two in due proportion, good iron is produced; may not this be owing to the circumstance of their containing different metals of the earths, which in compound alloy may be more oxidable than in simple alloys, and may be more easily separated by combustion?

Copper, M. Berzelius informs me, is hardened by silicium. In some experiments that I made on the action of potassium and iron on silex, the iron, as I have mentioned before, was rendered white, and very hard and brittle, but it did not seem to be more oxidable. Researches upon this subject do not appear unworthy of pursuit, and they may possibly tend to improve some of our most important manufactures, and give new instruments to the useful arts.

V. *Some Considerations of Theory illustrated by new Facts.*

Hydrogen is the body which combines with the largest proportion of oxygen, and yet it forms with it a neutral compound. This, on the hypothesis of electrical energy, would show that it must be much more highly positive than any other substance; and therefore, if it be an oxide, it is not likely that it should be deprived of oxygen by any simple chemical attractions. The fact of its forming a substance approaching to an acid in its nature, when combined with a metallic substance, tellurium, is opposed to the idea of its being a gaseous metal, and perhaps to the idea that it is simple, or that it exists in its common form in the amalgam of ammonium. The phenomena presented by sulphuretted hydrogen are of the same kind, and lead to similar conclusions.

Muriatic acid gas, as I have shown, and as is further proved by the researches of MM. Gay Lussac and Thenard, is a compound of a body unknown in a separate state, and water. The water, I believe, cannot be decompounded, unless a new combination is formed: thus it is not changed by charcoal ignited

in the gas by Voltaic electricity; but it is decompounded by all the metals; and in these cases hydrogen is elicited, in a manner similar to that in which one metal is precipitated by another; the oxygen being found in the new compound. This, at first view, might be supposed in favour of the idea that hydrogen is a simple substance; but the same reasoning may be applied to a protoxide as to a metal; and in the case of the nitro-muriatic acid, when the nitrous acid is decomposed to assist in the formation of a metallic muriate, the body disengaged (nitrous gas) is known to be in a high state of oxygenation.

That nitrogen is not a metal in the form of gas, is almost demonstrated by the nature of the fusible substance from ammonia, and (even supposing no reference to be made to the experiments detailed in this paper) the general analogy of chemistry would lead to the notion of its being compounded.

Should it be established by future researches that hydrogen is a protoxide of ammonium, ammonia a deutoxide, and nitrogen a tritoxide of the same metal, the theory of chemistry would attain a happy simplicity, and the existing arrangements would harmonize with all the new facts. The class of pure inflammable bases would be *metals* capable of alloying with each other, and of combining with protoxides. Some of the bases would be known only in combination, those of sulphur, phosphorus,* and of the boracic, fluoric, and muriatic acids; but the relations of their compounds would lead to the suspi-

* The electrization of sulphur and phosphorus goes far to prove that they contain combined hydrogen. From the phenomena of the action of potassium upon them in my first experiments, I conceived that they contained oxygen, though, as I have stated in the appendix to the last Bakerian lecture, the effects may be explained on a different supposition. The vividness of the ignition in the process appeared an evidence in favour of their containing oxygen, till I discovered that similar phenomena were produced by the combination of arsenic and tellurium with potassium. In some late experiments on the action of potassium on sulphur and phosphorus, and on sulphuretted hydrogen, and on phosphuretted hydrogen, I find that the phenomena differ very much according to the circumstances of the experiment, and in some instances I have obtained a larger volume of gas from potassium after it had been exposed to the action of certain of these bodies, than it would have given alone. These experiments are still in progress, and I shall soon lay an account of them before the society. The idea of the existence of oxygen in sulphur and

cion of their being metallic. The salifiable bases might be considered either as protoxides, deutoxides, or tritoxides: and the general relations of salifiable matter, to acid matter, might be supposed capable of being ascertained by their relations to oxygen, or by the peculiar state of their electrical energy.

The whole tenor of the antiphlogistic doctrines necessarily points to such an order; but in considering the facts under other points of view, solutions may be found, which, if not so simple, account for the phenomena with at least equal facility.

If hydrogen, according to an hypothesis to which I have often referred, be considered as the principle which gives inflammability, and as the cause of metallization, then our list of simple substances will include oxygen, hydrogen, and unknown bases only; metals and inflammable solids will be compounds of these bases, with hydrogen; the earths, the fixed alkalies, metallic oxides, and the common acids, will be compounds of the same bases, with water.

The strongest arguments in favour of this notion, in addition to those I have before stated, which at present occur to me, are: First, the properties which seem to be inherent in certain bodies, and which are either developed or concealed, according to the nature of their combinations. Thus sulphur, when it is dissolved in water either in combination with hydrogen or oxygen, uniformly manifests acid properties; and the same quantity of sulphur, whether in combination with hydrogen, whether in its simple form, or in combination with one proportion of oxygen, or a double proportion, from my experiments seems to combine with the same quantity of alkali. Tellurium, whether in the state of oxide or of hydruret, seems to have the same tendency of combination with alkali; and the alkaline metals, and the acidifiable bases, act with the greatest energy on each other.

Second, the facility with which metallic substances are revived, in cases in which hydrogen is present. I placed two plati-

phosphorus is, however, still supported by various analogies. Their being nonconductors of electricity is one argument in favour of this. Potassium and sodium, I find, when heated in hydrogen, mixed with a small quantity of atmospheric air, absorb both oxygen and hydrogen, and become nonconducting inflammable bodies analogous to resinous and oily substances.

na wires, positively and negatively electrified from 500 double plates of six inches, in fused litharge; there was an effervescence at the positive side, and a black matter separated at the negative side, but no lead was produced; though when litharge moistened with water was employed, or a solution of lead, the metal rapidly formed: the difference of conducting power may be supposed to produce some difference of effect, yet the experiment is favourable to the idea, that the presence of hydrogen is essential to the production of the metal.

Third, oxygen and hydrogen are bodies that in all cases seem to neutralize each other, and therefore in the products of combustion it might be expected that the natural energies of the bases would be most distinctly displayed, which is the case; and in oxymuriatic acid, the acid energy seems to be blunted by oxygen, and is restored by the addition of hydrogen.

In the action of potassium and sodium upon ammonia, though the quantity of hydrogen evolved in my experiments is not exactly the same as that produced by their action upon water; yet it is probable that this is caused by the imperfection of the process;* and supposing potassium and sodium to produce the same quantity of hydrogen from ammonia and water, the circumstance, at first view, may be conceived favourable to the notion that they contain hydrogen, which under common circumstances of combination will be repellant to matter of the same kind: but this is a superficial consideration of the subject, and the conclusion cannot be admitted; for on the idea that in compounds containing gaseous matter, and perhaps compounds in general, the elements are combined in uniform proportions; then whenever bodies known to contain hydrogen are decomposed by a metal, the quantities of hydrogen ought to be the same, or multiples of each other. Thus, in the de-

* There seems to be always the same proportion between the quantity of ammonia which disappears, and the quantity of hydrogen evolved; *i. e.* whenever the metals of the alkalies act upon ammonia, supposing this body to be composed of three of hydrogen, and one of nitrogen, in volume, two of hydrogen and one of nitrogen remain in combination, and one of hydrogen is set free. And it may be adduced as a strong argument in favour of the theory of definite proportions, that the quantity of the metals of the alkalies and nitrogen, in the fusible results, are in the same proportions as those in which they exist in the alkaline nitrates.

composition of ammonia by potassium and sodium, two of hydrogen and one of nitrogen remain in combination, and one of hydrogen is given off; and in the action of water on potassium to form potash, the same quantity of hydrogen ought to be expelled. From my analysis* of sulphuretted hydrogen, it would appear, that if potassium in forming a combination with this substance sets free hydrogen, it will be nearly the same quantity as it would cause to be evolved from water. And if the analysis of Mr. Proust and Mr. Hatchett, of the sulphuret of iron, be made a basis of calculation, iron, in attracting sulphur from sulphuretted hydrogen, will liberate the same proportion of hydrogen as during its solution in diluted sulphuric acid; and taking Mr. Dalton's law of proportion, the case will be similar with respect to other metals: and if such reasoning were to be adopted, as that metals are proved to be compounds of hydrogen, because, in acting upon different combinations containing hydrogen, they produce the evolution of equal proportions of this gas, then it might be proved that almost any

* The composition may be deduced from the experiments in the last Bakerian lecture, which show that it contains a volume of hydrogen equal to its own. If its specific gravity be taken as 35 grains, for 100 cubical inches, then it will consist of 2·27 of hydrogen, and 32·73 of sulphur. When sulphuretted hydrogen is decomposed by common electricity, in very refined experiments, there is a slight diminution of volume, and the precipitated sulphur has a whitish tint, and probably contains a minute quantity of hydrogen. When it is decomposed by Voltaic sparks, the sulphur is precipitated in its common form, and there is no change of volume; in the last case the sulphur is probably ignited at the moment of its production. In some experiments lately made in the laboratory of the Royal Institution, on arseniuretted and phosphuretted hydrogen, it was found that when these gases were decomposed by electricity, there was no change in their volumes; but neither the arsenic nor the phosphorus seemed to be thrown down in their common state; the phosphorus was dark-coloured, and the arsenic appeared as a brown powder, both were probably hydrurets: this is confirmed likewise by the action of potassium upon arseniuretted and phosphuretted hydrogen; when the metal is in smaller quantity than is sufficient to decompose the whole of the gases, there is always an expansion of volume; so that arseniuretted and phosphuretted hydrogen contain in equal volumes, more hydrogen than sulphuretted hydrogen, probably half as much more, or twice as much more. From some experiments made on the weights of phosphuretted and arseniuretted hydrogen, it would appear that 100 cubic inches of the first weigh about 10 grains, at the mean temperature and pressure, and 100 of the second about 15 grains.

kind of matter is contained in any other. The same quantity of potash, in acting upon either muriate, sulphate, or nitrate of magnesia, will precipitate equal quantities of magnesia; but it would be absurd to infer from this, that potash contained magnesia, as one of its elements; the power of repelling one kind of matter, and of attracting another kind, must be equally definite, and governed by the same circumstances.

Potassium, sodium, iron, mercury, and all metals that I have experimented upon, in acting upon muriatic acid gas, evolve the same quantity of hydrogen, and all form dry muriates; so that any theory of metallization, applicable to potash and soda, must likewise apply to the common metallic oxides. If we assume the existence of water in the potash, formed in muriatic acid gas, we must likewise infer its existence in the oxides of iron and mercury, produced in similar operations.

The solution of the general question concerning the presence of hydrogen in all inflammable bodies, will undoubtedly be influenced by the decision upon the nature of the amalgam from ammonia, and a matter of so much importance ought not to be hastily decided upon. The difficulty of finding any multiple of the quantity of oxygen, which may be supposed to exist in hydrogen, that might be applied to explain the composition of nitrogen from the same basis, is undoubtedly against the simplest view of the subject. But still the phlogistic explanation, that the metal of ammonia is merely a compound of hydrogen and nitrogen; or that a substance which is metallic can be composed from substances not in their own nature metallic, is equally opposed to the general tenor of our chemical reasonings.

I shall not at present occupy the time of the society by entering any further into these discussions; hypothesis can scarcely be considered as of any value, except as leading to new experiments; and the objects in the novel field of electrochemical research have not been sufficiently examined to enable us to decide upon their nature, and their relations, or to form any general theory concerning them which is likely to be permanent.

Explanation of the Figures.

Fig. 1. The apparatus for electrizing potassium in gases. A the glass tube. B the wire negatively electrified. C and D the cup and wire positively electrified.

Fig. 2. The apparatus for decomposing water, out of the contact of air, page 148. AA the cones containing the water. BBB the tubes for conveying the gas. C and D the pneumatic apparatus.

Fig. 3. The apparatus for decomposing and recomposing water under oil. CC the wires for communicating the Voltaic electricity. DD the wires for producing the explosion. B the tube. A the vessel containing it. *a*, *d*, *c*, the level of the different fluids.

Fig. 4. The apparatus for exposing water to the action of ignited potash and charcoal, out of the contact of air. A the tube for water. B the iron tube. C the receiver for the ammonia. D the pneumatic apparatus.

Fig. 5. The apparatus for the decomposition of ammonia.

Fig. 6. A Voltaic apparatus, being one of the 200 which compose the new Voltaic battery of the Royal Institution. For the construction of this battery, and of other instruments applicable to new researches, a fund of upwards of 1000*l.* has been raised by subscription, from members of the Royal Institution. As yet, the whole combination has not been put into action; but reasoning from the effects of that part of it which has been used, some important phenomena may be expected from so great an accumulation of electrical power.

Observations on Albumen, and some other Animal Fluids; with Remarks on their Analysis by electrochemical Decomposition.

BY WILLIAM BRANDE, F. R. S.

Communicated by the Society for the Improvement of Animal Chemistry.*

I. *Observations on Mucus, and on the Composition of liquid Albumen.*

The results obtained from the chemical analysis of the intervertebral fluid of the spalus maximus, an account of which is annexed to Mr. Home's paper "On the nature of the intervertebral substance in fish and quadrupeds,"† led me to undertake a series of experiments on mucus, in order to examine the properties of that secretion in its pure state, and to ascertain how far it might be capable of conversion into modifications of gelatine and albumen.

1. Saliva was the first source of mucus to which I directed my attention.

In order to separate the albumen, which Dr. Bostock's analysis has shown it to contain,‡ it was agitated for a short time with an equal quantity of pure water; the solution was then boiled and filtered. I considered the clear fluid, which had passed the filter, as a solution of nearly pure mucus; but found, on applying to it the tests of nitrate of silver, and acetate of lead, that it still contained a very considerable proportion of saline matter. The precipitate consisted of muriate and phosphate of silver and lead, in combination with a little animal matter, the odour of which was perceptible on exposing it to heat after it had been washed and dried.

One thousand grains of saliva afforded, by careful evaporation in a water bath, a residuum weighing one hundred and eighty grains, from which twenty grains of saline matter, consisting of phosphate of lime and muriate of soda, were obtained by incineration.

2. The mucus from the trachea, and that of the oyster were next examined; but here the proportion of saline matter was

* Philosophical Transactions for 1809, p. 373.

† Philos. Trans. 1809; or Journal, vol. XXV. p. 214.

‡ Nicholson's Journal, vol. XIV. page 149.

greater than in the former case, although no traces of albumen could be detected by the usual tests of heat, alcohol and acids.

Finding, therefore, that the reagents employed to detect mucus* act principally upon the salts which it contains, and not merely upon the secretion itself, it became an object of some importance to find out a method of depriving it of its saline ingredients, by such means as should not affect the mucus. Decomposition by electricity immediately occurred to me, as the most likely means of attaining the object I had in view.

For this purpose, I procured three glass cups, each capable of holding rather more than a measured half ounce of water; one of these was filled with a mixture of equal parts of saliva and pure water; this was connected with the other two, containing pure water, by filaments of moistened cotton. The water in one of the cups was rendered positive, that in the other negative, by a Voltaic battery of one hundred and twenty four-inch double plates, charged with a solution of nitro-muriatic acid, in the proportion of one part of the mixed acid to thirty parts of water.† By continuing this process, I hoped to decompose the saline ingredients of the saliva, to collect the acid matter in the positive, and the alkaline matter in the negative cup, and thus to leave the mucus and albumen in the centre vessel (free from the salts which they contain in their natural state), and to have separated them by boiling distilled water, which would then have afforded a solution of pure mucus.

When the action of the battery had been continued for about ten minutes, a considerable quantity of a white substance surrounded, and adhered to, the cotton on the negative side of the circuit, whereas on the positive side no such effect had taken place.

I could not at first account for this appearance, conceiving that, if it depended on the coagulation of albumen held in solution in the saliva, it would have taken place at the positive pole, in consequence of the acid there separated.

* Nitrate of silver and acetate of lead. Vide Thomson's System of Chemistry, Vol. V. page 500, 3d edition; and Nicholson's Journal XI—251.

† It was conceived, that this electrical power, though sufficient for the decomposition of the salts, would not materially affect the animal matter.

To ascertain this point, an experiment was made on the albumen of an egg.

When the conductors from the same battery were brought within two inches of each other in this fluid, an immediate and rapid coagulation took place at the negative wire, while only a thin film of albumen collected at the positive wire, where its appearance was readily accounted for, by the separation of a little acid, which, reacting on the albumen, would render it solid; but the cause of the abundant coagulation at the negative pole was not so obvious.

This result I mentioned to Mr. Davy, who immediately offered an explanation of it, by supposing the fluidity of albumen to depend upon the presence of alkaline matter, the separation of which, at the negative pole, would cause it to assume a solid form. I had only to follow up this idea, and shall proceed to state the principal experiments, which were undertaken to establish so probable an opinion.*

1. When coagulated albumen, cut into small pieces, is boiled in distilled water, it imparts a viscidity to that fluid, showing that something is retained in solution.

Two hundred grains of the coagulated albumen of an egg were repeatedly washed and triturated in four ounces of distilled water, which was afterwards separated by a filter, and evaporated to about one fourth of its original bulk. It was then examined by the usual tests, and was found evidently alkaline; it converted the yellow of turmeric to a pale brown, and restored the blue colour to litmus paper, reddened by vinegar; but it did not appear to effervesce on the addition of a dilute acid.

On evaporating this alkaline fluid to dryness, by a gentle heat, a viscid substance, soluble in water, was obtained. This solution was rendered slightly turbid by an acid; and by the application of electricity, from sixty four-inch double plates, a copious coagulation took place at the negative pole.

* On referring afterwards to Dr. Thomson's System of Chemistry, (Vol. V. page 491), I find, that a very similar explanation of the coagulation of albumen has been offered by that author, which the following experiments will likewise confirm.

So that water, in which the coagulated white of egg has been boiled, is in fact an extremely dilute alkaline solution of albumen.

This enables us also to explain why albumen becomes coagulated simply by heat.

When the coagulated white of egg is cut into pieces, a small quantity of a brown viscid fluid gradually separates from it, as has been observed by Dr. Bostock in his paper on the primary animal fluids.* This I find to consist principally of an alkaline solution of albumen. It reddens turmeric, and coagulates abundantly on the application of negative electricity.

It appears, therefore, that the white of an egg, in its fluid state, is a compound of albumen with alkali and water; that when heat is applied to it, the affinities existing between these bodies are modified; that the alkali, before in chemical combination with the albumen, is transferred to the water, and that this separation causes the coagulation of the albumen: the aqueous alkaline solution, which is thus formed, reacts upon the coagulated albumen, of which it dissolves a small portion, and then appears in the form of the brown viscid fluid already noticed.

The coagulation of albumen by alcohol and by acids may be explained by a reference to the principles already laid down.

1. Five hundred grains of the white of egg were agitated with two ounces of pure alcohol; an immediate coagulation resulted, which was rendered more perfect by the application of a very gentle heat. The liquor was separated from the coagulum by filtration, and evaporated to half its bulk; when the usual tests were now applied, alkaline matter was abundantly indicated.

In this instance then, the albumen, in passing from the liquid to the solid state, gives its alkali to the alcohol.†

2. When acids are applied to albumen, these effect its coagulation from the same cause: they render it more rapidly and

* Nicholson's Journal, Vol. XI—246.

† When albumen is coagulated by alcohol, it does not become so perfectly solid as in most other instances, because the separation effected by the relative affinities is not so complete.

more perfectly solid, on account of their superior affinity for the alkali.

The following experiments were instituted with a view to ascertain the nature and quantity of the alkaline matter which exists in liquid albumen.

1. Five hundred grains of the liquid white of egg were mixed with two ounces of distilled water, and exposed for half an hour to a temperature of 212°. The fluid was then separated by a filter, and the coagulated albumen cut into small pieces, and repeatedly washed with boiling distilled water. The filtrated fluid was evaporated to half an ounce by measure; it had a saline taste, it was somewhat turbid, and slightly alkaline; on cooling, it gradually deposited a few flakes of albumen: it was electrified positively in a small glass cup, connected by washed cotton to another similar vessel containing a little distilled water, negatively electrified by one hundred four-inch plates, charged with a solution of nitro-muriatic acid of the same strength as that employed in a former experiment, fresh portions of water being occasionally added in order to compensate for the loss by its decomposition.

When the electrization had been carried on in this way for one hour the cups were removed, and their contents examined.

The fluid in the negatively electrified cup acted rapidly on turmeric, rendering it deep brown. On evaporation and subsequent exposure to a low red heat, it afforded a residuum weighing 5·5 grains, which had the properties of soda, in a state approaching to purity.

The positive cup contained a little coagulated albumen, and an acid which was principally, if not entirely the muriatic, was held in solution by the water: it gave a very copious precipitate with nitrate of silver, which became speedily black on exposure to light. When saturated with carbonate of soda, and evaporated, it afforded a salt in small cubic crystals, from which the fumes of muriatic acid were developed by the action of the sulphuric.

This experiment shows, that, exclusive of soda in an uncombined state, fluid albumen contains some muriate of soda.*

* May not a submuriate of soda exist in fluid albumen?

We learn from the experiments of Mr. Hatchett, that minute quantities of other saline bodies are likewise present.*

In the foregoing experiments, I had generally employed from sixty to three hundred four-inch double plates of copper and zinc, but in subsequent researches, made with a view of ascertaining the action of lower powers, the effects of which I shall afterwards relate, I find that a battery of twenty-four three-inch double plates is sufficient to effect a perfect coagulation at the negative pole, even where the albumen is diluted with so large a quantity of water, as not to be detected by the usual tests.

II. *Observations on the Composition of some animal Fluids containing Albumen.*

Finding, from the experiments detailed in the preceding section, that albumen may exist in such states of combination, as not to be detected by the usual tests, but separable by electrical decomposition, I was induced to apply this mode of analysis to the examination of animal fluids in general.

1. *Saliva.*

When saliva is boiled in water, a few flakes of coagulated albumen are deposited; but this is by no means the whole quantity of albumen contained in the secretion, for on applying the test of negative electricity to the filtered fluid obtained after the separation of the albumen by heat, a copious coagulation and separation of alkali are produced at the negative pole. A

* After the destructive distillation of coagulated, dry, semitransparent albumen, there remained "a spongy coal of very difficult incineration; as "towards the end of the process it appeared vitrified, and glazed with a "melted saline coat, which was, however, easily dissolved by water. The "residuum was again exposed to a long continued red heat, and again treated with water, till, at length, a few scarcely visible particles remained, "which, as far as such small quantity would permit to be ascertained, "proved to be phosphate of lime. The portion dissolved by water (which "was by much the most considerable) consisted principally of carbonate, "mixed with a small quantity of phosphate of soda.

"Five hundred grains of dry albumen afforded 74·50 grains of coal, of "which 11·25 were saline matter."

See "Chemical experiments on Zoophytes, with some Observations on the "Component Parts of Membrane." Phil. Trans. 1800.

large portion of albumen may therefore exist in a fluid, incapable of separation by heat, and in the present instance not to be detected even by acids, these reagents producing no effect on the filtered solution just alluded to.

2. *Mucus of the Oyster.*

The solution of mucus obtained by agitating oysters in water exhibits to the usual tests no traces of albumen; but when acted upon by electricity from the Voltaic battery, a considerable and rapid coagulation takes place at the negatively electrified wire.

3. *Mucus of the Trachea, &c.*

The other varieties of mucus, as from the trachea, the nose, &c., agree with the former, in affording abundance of albumen by electric decomposition; whereas scarcely any traces of this substance can be detected by the tests of acids, heat, or alcohol.

In these experiments, alkaline matter was always evolved at the negative, and acid at the positive wire. Minute researches, made with a view of ascertaining the nature of the alkaline and acid matter thus evolved, showed the former to consist of soda, with traces of lime; the latter of muriatic acid, with traces of phosphoric acid in the cases of saliva, and mucus of the trachea and nose: the mucus of the oyster afforded only soda and muriatic acid.

On examining the proportions of alkali and acid, the former seemed always to predominate, although in the original fluids no traces of uncombined alkali (as in the white of egg) are to be detected.

These results lead to new ideas respecting the composition of mucus: Is it a peculiar combination of muriate of soda and albumen? or may it not be a compound of soda and albumen, in which the alkali is not separable by the usual modes of analysis, but which yields to the superior decomposing energy of electricity?

4. *Bile.*

An immediate coagulation took place in this secretion, at the negative conductor, the albumen being tinged through-

out of a green colour, arising from the colouring matter at the same time separated.

The relative proportion of albumen, separable by electricity from different specimens of ox-bile, was found to be liable to considerable variation, so that a detailed analysis of this fluid cannot be generally depended upon. I have found the albumen in bile to vary in quantity from 0·5 to three per cent.; and it is somewhat remarkable, that where there is a small quantity of albumen, there likewise the proportion of the resinous matter of bile is relatively small.

The electrochemical decomposition of this fluid affords, beside the results just mentioned, a considerable quantity of soda at the negative pole; and at the positive pole, a mixture of muriatic and phosphoric acids.

5. *Milk.*

In this fluid, the separation of albuminous matter at the negative pole is equally evident, though not so rapid, as in most other cases. The conductors from sixty four-inch double plates, highly charged, and immersed within four inches of each other in three ounces of cow's milk, during one hour, produced the appearance of curds and whey, the principal part of the curd being collected in the neighbourhood of the negative wire, and but little at the positive wire. When this experiment was so conducted, as to collect the products in separate vessels, the predominating ingredients in the contents of the negative cup were soda and traces of lime; and in the positively electrified vessel, a mixture of muriatic and phosphoric acids.

After such decomposition of milk, the serum still affords sugar of milk.

6. *The Liquor of the Amnios.*

An opportunity having offered of examining this secretion, from the human subject, in its pure and fresh state, I shall mention the general results of its analysis.

The liquor of the amnios is almost perfectly transparent, but on exposure to air becomes gradually turbid, and deposits a white flaky matter. It renders tincture of violets green, and while perfectly fresh does not affect litmus; but sulphuretted

hydrogen is soon evolved from it, and then it slightly reddens litmus. When heated, it becomes turbid, and lets fall flakes of coagulated albumen. Acids render it slightly turbid from the same cause.

Alkalis produce no change, unless when added in considerable excess: the odour of ammonia is then perceptible.

Electrical analysis afforded albumen and soda at the negative pole, and muriatic acid at the positive pole. Hence we learn, that the liquor of the amnios has the properties of a dilute solution of liquid albumen.*

7. *Pus.*

In the pus of a healthy sore, coagulation took place at both poles; most abundantly, however, at the negative pole. A slight degree of putrefaction having commenced in the pus which was examined, I did not pay particular attention to the other products of the experiment.

In concluding this section, it may be proper to remark, that the decomposition of liquid albumen by Voltaic electricity takes place in different ways, according to the power employed. With a comparatively high electrical power, the coagulation goes on rapidly at the negative pole, and only very slowly at the positive pole; whereas, with an extremely low power, the coagulation is comparatively rapid at the positive surface, an alkaline solution of albumen surrounding the negative pole. Thus, when the conductors from twenty-four four-inch double plates, highly charged, were brought within half an inch of each other, in a dilute solution of albumen (consisting of one part of albumen to six of water), the coagulation was considerably more abundant at the negative than at the positive pole: but when the conductors were removed from each other to a distance of eight inches, or when they remained at half an inch, being connected with a battery of six four-inch double plates only, the coagulation was only perceptible at the positive pole, in consequence of the acid there collected. Hence we may

* The difference in the results of the analysis given in the text, and that of Vauquelin and Buniva, most probably arises from the liquor of the amnios examined by those chemists not having been perfectly recent, and perhaps mixed with other secretions. See *Annales de Chimie*, XXXIII. p. 270.

infer, that a rapid abstraction of alkali is necessary to the perfect coagulation of albumen, since, in the cases above alluded to, the albumen remains in solution.

An Account of the Effects produced by a large quantity of Laudanum taken Internally, and of the Means used to counteract those Effects.

BY ALEXANDER MARCET, M. D. F. R. S.

One of the Physicians to Guy's Hospital.

From the Medico-Chirurgical Transactions for 1809.

ON the sixth of November last, Mr. Astley Cooper informed me, at four o'clock in the afternoon, that he had just seen a young man about eighteen years of age, who had taken, at ten o'clock in the morning, no less than *six ounces of laudanum*, the whole of which had remained in his stomach, and had brought on symptoms which appeared to threaten immediate dissolution. Mr. Cooper who did not see him till three o'clock, that is about five hours after the accident, acquainted me that he had made him swallow, at half past three o'clock, a solution of one drachm and a half of white vitriol, or sulphat of zinc, which had produced some nausea, and had made him vomit about one ounce and a half of fluid which had a strong smell of opium; notwithstanding which the lethargy had gradually increased, and he had at last fallen into a state of complete insensibility. Some mustard had also been administered, without any obvious effect.

Mr. Cooper having requested me to see this gentleman, and to take any farther steps which circumstances might suggest, I called upon him a few minutes after four o'clock, when I found him on the floor, resting on his knees with his body leaning forwards and supported by two friends, who, as I afterwards learnt, were in the act of laying him down in order to let him die in peace. His head was hanging lifeless on his breast, with his eyes shut and his countenance ghastly. His

respiration was slow and sonorous, like the apoplectic breathing. His hands were cold, and the pulse beat from ninety to ninety-six strokes in a minute, in a feeble and irregular manner. All the muscles of his body were in a state of extreme relaxation, and the flesh of his arms in particular felt singularly soft and inelastic.

Blue vitriol or sulphat of copper being the first remedy that occurred to me as likely to produce vomiting, about half a drachm of this substance was quickly dissolved in water, and the patient being abruptly raised from the floor and strongly shaken, he opened his eyes, and seemed disposed to offer a kind of feeble resistance to the attempt he saw us about to make. We succeeded, however, in pouring into his throat about half the quantity of vitriolated copper, just mentioned, that is a dose equivalent to fifteen grains, which he swallowed with a kind of agonizing effort. Immediately after this, his countenance, which had been for an instant roused, became still more ghastly. But he had scarcely taken the dose one minute when he suddenly threw up a large quantity of a brownish fluid, which had a strong smell of laudanum, and was immediately followed by two or three more gushes of the same liquid, the whole amounting to between one and two pints. He was then made to swallow some warm water, and was dragged from one room to another, with a view to counteract the state of torpor above described. His limbs at first were quite passive and lifeless, but in a few minutes he began to rest, in some degree, on his legs, with the assistance of his friends. He continued, however, with his eyes closed, (unless roused by a loud and sudden call) his pupils dilated, and his breathing apoplectic. I strongly recommended to his friends, who fortunately were extremely active and intelligent, that he should be kept incessantly on his legs, and in constant motion about his room.

When I called again at nine o'clock in the evening, I found him so far recovered as to walk about the room supported by a friend. His countenance appeared more natural, and he was able, when urged by questions, to answer by monosyllables like a man in a state of extreme intoxication. He had vomited once or twice more since I had left him in the afternoon. He gave me to understand that he felt cold at the pit of his sto-

mach, hot on the surface of his body, and cold in the extremities. In spite of this improvement he still slept profoundly, and snored loudly, even whilst in the act of walking about the room; and when forcibly roused, he opened his eyes for an instant and fell again into a deep slumber. Mr. Cooper saw him also in the evening, and we both agreed in recommending that he should be kept in the same state of forced activity through the night, that he should take frequent doses of assafœtida with volatile alkali, of camphor, or even of musk, if other stimulants did not appear sufficiently active. It was also agreed that his head should be blistered and sinapisms applied to his feet. Some tea and coffee, and likewise lemon juice, (of which he had taken small quantities during the evening with very good effect,) were directed to be frequently offered to him, and we strongly recommended that he should never be longer than half an hour at a time during the night, without being roused, in order to take either some medicines or some liquid nourishment.

On calling the next morning, (November 7th) I learnt that he had got so much better by twelve o'clock at night, that his friends had found it unnecessary to apply the blister, and that a few doses of camphorated julep, with assafœtida, were the only remedies he had taken. But he had frequently sipped small quantities of tea, coffee, and lemon juice, all of which were extremely grateful to him. He was prevented from going to sleep, and kept in constant agitation by his friends, till six o'clock in the morning, when he was allowed to go to bed.

On the following morning, between nine and ten o'clock, I found him still asleep; but on my approaching his bed, he readily awoke being rather confused at first, but soon recovering himself, he said, he supposed he had slept three or four hours, which was exactly the case, and complained of his throat being sore, as if excoriated. He observed also, that a glyster which had been administered during the night, was coming away by degrees, mixed with fæces, without his being sensible of it, or able to prevent it.

On the following day (November 8,) he was able to take a walk out of doors. His appetite was not yet returned; but he was not averse to taking food. He still complained of soreness

in his throat, and also at the root of his tongue, both of which were evidently the effects of the caustic antidotes he had swallowed. He had had no passage through his body since his illness, except that which was occasioned by the glyster. He still looked sallow and dejected, and complained of an uneasy sensation at the pit of his stomach, not, however, amounting to pain. I advised him to take a dose of rhubarb and calomel.

In a few days after this he was perfectly recovered. The circumstances which occasioned this accident having no sort of connexion with the symptoms which it produced, I have thought it useless to enter into a detailed account of them. But I should not omit to observe, that after a strict inquiry into all particulars, it was perfectly ascertained that the quantity of laudanum taken was actually six ounces.

The inference which may naturally be drawn from this case, and which, indeed, induced me to consider it as sufficiently important to be communicated to the Society, is, that neither the lapse of several hours after an accident of this kind, nor the failure of various means of relief, should prevent the administration and diligent repetition of the most powerful emetics.

A Case of Artificial dilatation of the Female Urethra, &c. &c.

BY H. L. THOMAS, ESQ. F. R. S.

From the Medico-Chirurgical Transactions for 1809.

SUNDAY, April 26th, I was called into the country to see a lady, thirty-four years of age, and the mother of several children. On the morning of that day she had, without any assignable cause, laboured under a suppression of urine; as there was no regular assistance at hand, the husband (whose readings on this subject had been pretty extensive), took upon himself to afford her relief. With this view he introduced an ivory ear-picker into the meatus urinarius, and the water immediately flowed, but as the quantity evacuated did not quite equal his expectations, the instrument was introduced a second time,

when it escaped from his fingers, and suddenly slipt, as he expressed it, with a "jerk into the body." When I saw her, six hours after, she was free from pain, and with no other unpleasant symptom than those which might be expected from the agitated state of her mind. Upon introducing the sound the extraneous substance was readily detected, and was as easily laid hold of by a pair of fine polypus forceps, but I found every attempt to extract it was attended with considerable pain, and followed by a slight discharge of blood, appearances which gave me reason to imagine that the instrument lay across the bladder, with its pointed extremities entangled between the fasciculi of muscular fibres, and that much mischief might be produced by any farther attempts at extraction. I therefore merely ordered an enema to be administered, and directed her to drink plentifully of diluting mucilaginous liquids; I also gave some directions respecting the position of the body at the time of evacuating the urine, which were to be such as to render the orifice of the bladder the most depending part.

On the Monday at two o'clock, I found that she had passed a restless night, and was much indisposed; pulse frequent, face flushed, and the tongue furred; a slight degree of tenderness was beginning to pervade the whole of the hypogastric region. This situation of the patient clearly pointed out the propriety of a speedy removal of the cause of irritation; and as I came provided with the bistoire cachée, I proposed opening into the bladder immediately; this proposition was, however, absolutely rejected by the husband, from the knowledge he had of a case of lithotomy in a female, who ever afterwards suffered from an incontinency of urine.

With some hesitation, and I must confess with little prospect of success, I introduced a piece of sponge tent into the urethra, three inches long, and somewhat thicker than the full sized female sound, leaving directions for its removal at the end of two hours, or even sooner, if attended with much pain or distress. I took care to prevent the possibility of the sponge separating, and any part escaping into the bladder, by passing a strong string through its whole length secured at both extremities by a double knot.

In the evening I was informed that she had borne the tent for two hours, without any great degree of irritation taking place, and that during the whole time the urine gradually escaped. I sent another piece of the tent somewhat larger than the first, desiring it to be introduced at twelve o'clock the following morning, and allowed to remain in the urethra till I came to her at two. On my arrival I found her tolerably well, and upon withdrawing the tent I passed the fore finger of my left hand into the bladder, where I felt the ear-picker lying across the cervix. At first I conceived it impossible, without laceration to disengage it from the position in which it appeared so firmly fixed, but by turning my hand, and insinuating the point of the fore finger underneath, and towards the blunt extremity, it was readily dislodged, and escaped through the urethra, by the side of my finger. It measured three inches in length, one end being considerably more pointed than the other. I kept my finger engaged in the bladder for (I should guess) five minutes after the expulsion of the ear-picker, solely with a view of attending to the contractile force of that viscus. The sensations I experienced did not in any way accord with the ideas I had formed, for instead of my finger being firmly compressed, as I expected, by the sphincter muscle and bladder generally, I found every where nothing else than a soft pulpy yielding substance, totally insensible to any stimulus I could produce by the finger nail; this inability to contract, may perhaps be accounted for by the partial distention of the bladder for so long a time by the ear-picker. The same agent, however, cannot be assigned as the cause of the general relaxation which had taken place in the urethra, and which was so very complete, that before I withdrew my finger, I believe, had the case required it, both thumb and finger would have passed into the bladder without the smallest difficulty.

On the following day the patient was so well as to preclude even an excuse for any farther examination of the parts; but I was given to understand that the involuntary discharge of urine continued only six hours, and that now she possessed the full powers of expelling it without any other inconveniency than a slight degree of scalding in its passage along the urethra.

My view in laying this case before the Society is to show how readily, and with what little pain the female urethra will admit of dilatation. There is no novelty in the mode of accomplishing the object of distention. Near a century back it was proposed by Douglas; he not only recommended sponge for this purpose, but also dried gentian root, as being more gradual in its expansion, and better fitted for the purpose.

Mr. Bloomfield has given the case of a young girl where he effected the dilatation by introducing the cœcum of a small animal, in a collapsed state, into the bladder, filling it afterwards with warm water, by means of a syringe. This was gradually withdrawn as the cervix vesicæ opened, and in a few hours the dilatation was so far accomplished as to allow the calculus to pass through.

Why some of these methods have not been more generally adopted I cannot say, perhaps the incontinency of urine which occasionally has been observed to succeed to great distention of the urethra is the reason of their having been laid aside; with what propriety, however, may be questioned, for I believe it will be found that this unpleasant symptom as frequently occurs after the operation of lithotomy, as it is now usually performed.

We have many well authenticated cases on record where calculi, of a size larger than a hen's egg, have been expelled from the bladder by the expulsive efforts of its own muscles. Heister has related from good authority several instances of the kind, a very remarkable case is also given by Dr. Molineux, in the early part of the Philosophical Transactions, where a stone was voided by a woman, "the circumference of which "measured the longest way $7\frac{6}{10}$ inches, and round about where "it was thickest $5\frac{3}{4}$ inches, its weight near $2\frac{1}{2}$ ounces troy."

If these relations can be credited, and there is no reason why they should not, I can hardly conceive any cause in a young and healthy female subject, and where the bladder is free from disease, why a very large stone may not be extracted without the use of any other instrument than the forceps, the urethra having first been sufficiently dilated by means of the sponge tents; for this purpose the blades of the forceps need not be so thick and strong as those commonly employed.

It may be often observed, that muscular fibres, when once stretched to a certain extent, (i. e.) beyond their capability of immediately reacting, will admit of very considerable elongation without tearing, and yet shall, when the cause is removed, perfectly and very speedily recover their original powers. When the luxation of the femur takes place downwards, we observe the great gluteus muscle upon the stretch, almost to breaking: before the reduction can be accomplished the fibres will be still farther elongated, yet after this violence of extension, laceration of the fibres hardly ever happens, and when the muscles are again restored to their former situation, their functions become as perfect in a few days as if no injury had been sustained. That sphincter muscles are possessed of similar powers, the above case will, I think, in great measure prove, in addition to which I beg leave to subjoin the following.

A gentleman of an inactive and sedentary disposition had for many years suffered from constipated bowels, which increased to that degree that the most active cathartics failed in producing the desired effect. By the advice of a practitioner, whom he consulted in Paris, he daily introduced into the rectum a piece of flexible cane (about a finger's thickness), where it was allowed to remain until the desire for evacuating the fæces came on. This plan succeeded so well that for more than a twelve month he never had occasion to resort to any other means. One morning, being anxious to fulfil a particular engagement in good time, in his hurry he passed the stick farther up, and with less caution than usual, when it was suddenly sucked up into the body, beyond the reach of his fingers. This accident, however, did not interrupt the free discharge of the fæces, and the same evacuation regularly took place every day, whilst the stick remained in the gut. It was seven days afterwards when I first saw him; he was in a very distressed state, with every symptom of fever, tension of the abdomen, and a countenance expressive of the greatest anxiety. His relatives and friends were totally ignorant of the real nature of his case; and nothing less than the urgency of his sufferings, could ever

have prevailed upon him to disclose it to me. Such were his feelings on the occasion, that a violent hysteric fit was brought on by the mere recital of what he termed his folly.

Upon examination with my finger, per anum, no part of the cane could be discovered; but one end of it was readily felt projecting (as it were) through the parietes of the abdomen, midway between the ilium and the umbilicus on the right side. The slightest pressure upon this part gave him exquisite pain.

After repeated trials I was at length enabled, with a bougie to feel one extremity of the stick lodged high up in the rectum; but without being able to lay hold of it with the stone forceps. To allay the irritation for the present, an emollient clyster, with tinct. opii. ʒij, was given, which passed without the least impediment, and did not return. On the next examination, two hours after, I found the sphincter ani considerably dilated, and by a continued perseverance to increase it, the relaxation became so complete, that in about twenty minutes I was enabled to introduce one finger after the other, until the whole hand was engaged in the rectum.

I found the end of the stick jammed in the hollow of the sacrum, but by bending the body forward it was readily disengaged, and extracted. Its length was nine inches and a half, with one extremity very ragged and uneven.

For several days after, the situation of the patient was highly critical, the local injury, joined to the perturbation of his mind, brought on symptoms truly alarming. At length I had the satisfaction to witness his complete recovery; and he has ever since (more than two years ago) enjoyed good health, and the regular action of the bowels, without the assistance of medicines, or any other aid.

Observations on the Digestion of the Stomach after Death.

BY ALLAN BURNS,

Member of the Royal College of Surgeons in London, and Lecturer on Anatomy and Surgery in Glasgow.

From the Edinburgh Medical and Surgical Journal for 1810.

WHILE inspecting dead bodies, the stomach has repeatedly been found eroded, in subjects where the previous symptoms gave no reason to expect such an occurrence. Yet, till lately, appearances were ascribed by those who saw them to disease; and the cases were described with peculiar care, because they were thought to illustrate the extensive disorganization which might take place, without being accompanied by characteristic symptoms, and without causing death. This is hardly to be wondered at, when we remember, that our forefathers entertained mistaken, and too frequently preposterous, notions regarding the functions of the stomach. They knew that the food was digested in this viscus; but instead of investigating the laws which regulate this process, they wasted their time in idle speculations, about the supposititious effects of fermentation, trituration, and putrefaction; the agency of the gastric juice was by them completely neglected. Mr. John Hunter, and some enlightened physiologists, his contemporaries, discarding the influence of imaginary causes, began to examine the digestive process. In the course of this inquiry, new facts presented themselves; the stomach was found, not only to afford a lodgment to the food, while the process of digestion was going on, but also it was discovered, that, either by its own vessels, or by those at the termination of the œsophagus, a peculiar fluid was secreted, a fluid possessed of the curious property of dissolving the food, and reducing it to a pultaceous mass. This was a material improvement in the history of the function of digestion; it was a new fact, which the comprehensive mind of Mr. Hunter soon saw might be advantageously employed in illustration of several of the phenomena observed during dissection. He especially applied it to the explanation of some of the cases of erosion of the stomach, and he satisfactorily de-

monstrated, that generally this was produced by agents acting after death. He likewise proved, beyond a doubt, that during life the stomach resists the action of the fluid which dissolves the food; but he, at the same time, made it plainly appear, that the gastric juice is, after the death of the animal, capable of dissolving the stomach which formed it, and which had, during life, resisted its action by its vital principle. Dr. Adams imagines, that Mr. Hunter believed that it was only under certain circumstances of death that this digestion of the stomach would actually happen. "But, for this purpose, he saw that the animal must be in health immediately before death, otherwise, neither the quantity nor quality of the secretion would be equal to the purpose. He was confirmed in this by the instances in which he saw the stomach digested. Both were men who had died from a violent death; both had been previously in sufficient health to eat a hearty meal. The fair inference from these was, that when men die of disease, the appetite usually ceases, and probably the secretion of the gastric juice also."* Dr. Adams will, I trust, excuse me, when I point out an error he has committed. Mr. Hunter, in his observations, details the history of three examples in which the stomach was digested. Two of the men died shortly after having had their skulls fractured, and the third was "a man who had been hanged."† Dr. Adams also appears to me to have drawn an inference from the two cases which he does notice, by no means warranted by the general tenor of Mr. Hunter's essay. Although the individual cases which Mr. Hunter adduces to prove the fact of the solution of the stomach after death, were subjects who had been deprived of life by violence; still, in no part of his paper, does he limit the action of the gastric juice on the stomach, to such as have been killed by violence. Indeed, it is so much the reverse, that he inculcates, that there are few dead bodies in which the stomach is not in some measure dissolved. Nay, he declares, "that there is frequently a considerable aperture made in this viscus, and, in many subjects, this digestive power extends much farther than through the stomach. I have often

* Adams's Observations on Morbid Poisons, 2d edition, p. 30.

† Hunter's Observations on Digestion, p. 185.

found that, after it had dissolved the stomach at the usual place, the contents of the stomach had come into contact with the spleen and diaphragm, had partly dissolved the adjacent side of the spleen, and had dissolved the diaphragm quite through, so that the contents of the stomach were found in the cavity of the thorax, and had even affected the lungs in a small degree."* I would here beg leave to inquire, is it probable, that one so accurate in his statement, and so faithful in his description, as Mr. Hunter must be allowed, even by his opponents, to have been, would have used such expressions had he merely seen the three cases which he particularizes. It is quite incredible, nor can I conceive how Dr. Adams came to fall into such an error. I have from Mr. Hunter's own paper proved, that he had really seen subjects having the stomach digested, besides these he has specified; but, as these cases were the first which came under his observation, they seem to have made the most lasting impression. From this circumstance, and from the manner in which Mr. Hunter wrote, many have embraced the opinion adopted by Dr. Adams, that solution of the coats of the stomach never does take place, except where the person has been suddenly deprived of life. Yet I do not conceive that Mr. Hunter either sanctions this opinion, or that he was the one who brought it into notice. We find nothing of this kind in his essay, where it is clearly stated, that he had met with digestion in the stomach in some who had died a natural death. Can any thing be more explicit than the statement which Mr. Hunter gives, when he says, that for some time he was at a loss how to explain these solutions of the stomach, especially as "they were most frequent in those who had died of violent deaths." No man will believe that Mr. Hunter would have expressed himself in this way, if he had never met with an instance of digestion of the stomach, except after death from violence. Were this point merely of a speculative nature, and were its decision merely a matter of curiosity, I would consider the time employed in discussing it, as little better than misspent; but when I consider, that it is one on which the practitioner may be called to give his opinion, in a situation where that opi-

* Hunter's Observations, p. 186

nion may determine the fate of a criminal, I cannot but view it as an important question, which it is the duty of every one to elucidate to the utmost of his ability. This consideration, and having had some opportunity of investigating this subject, has induced me to collect the observations which I had made during the dissection of those bodies in which I found the stomach digested. These observations have led me to conclude, that digestion of the stomach after death, is neither so rare in its occurrence as some have imagined, nor is it confined to such subjects as had been, previous to death, in a healthy condition; and they have also demonstrated, that other parts of the stomach, besides the large end, may be acted on by the gastric juice.

That the digestion of the coats of the stomach after death is not a very rare occurrence, I think myself authorized to infer, from my having examined nine bodies in which the solution had proceeded to such an extent as to have made holes of considerable size through that viscus. Besides these nine instances in which the digestion of part of the stomach was complete, I have had occasion to see, in opening this viscus, various degrees of dissolution of its villous coat.* In some of these cases the appearances presented were similar to those described by Mr. Hunter, but in others there were considerable differences. In the bodies examined by Mr. H. "the parts acted on appear smooth, thin and more transparent, and the vessels will be seen ramifying in its substance; and, upon squeezing the blood from the larger to the smaller branches, it will be found to pass out at the digested ends, and appear like drops on its inner surface."† This condition of the vessels, which by Mr. H. is made a prominent feature in those cases where the digestion of the stomach has been produced by the gastric juice, does not invariably accompany such solution. In three of the subjects which I have dissected, there was no appearance of vessels ramifying

* It will generally be found that, where the coats of the stomach are softened by the gastric juice, the vessels are unable to resist the force of the syringe in injecting the body. In such subjects we therefore find the cavity of the stomach filled with wax, and we likewise see masses of it collected between the coats of the viscus.

† Hunter's Observations, p. 186.

on the coats of the stomach. In accounting for the want of these vessels, some have given one explanation, while others have attempted a different one. To me it has appeared, that time has been wasted on this inquiry. We are not to look for an explanation of this difference from Mr. Hunter's description, as arising from the particular part of the stomach acted on in the different cases; neither are we to imagine that the stage of the process at which we examine the body will assist us in this investigation.* We are rather to obtain an explanation of this fact, from contemplating the differences of condition of the different individuals at the time of death. Were not the subjects whose cases Mr. Hunter has detailed, men cut off by violence, in the fulness of health, their stomachs, at the time, excited by the stimulus of food to vigorous action, and the process of digestion at the instant of death going on briskly, circumstances under which it is reasonable to infer that all the bloodvessels would be filled with blood, which it is evident, from the nature of the causes depriving them of life, would be detained in the veins? This being the state of his subjects at the moment of death, we shall not wonder that, when he afterwards opened the bodies, he could squeeze the blood from the digested ends of the vessels. In three of my subjects in whom I found the stomach digested, and this ap-

* It has been supposed, that the appearance of the vessels described by Mr. Hunter, will only be met with in solution of the great end of the stomach. At this point the stomach receives the vasa brevia from the splenic vessels, consequently the bloodvessels are here both larger and more numerous than elsewhere, and hence, when this viscus is dissolved, they are found in the state noticed by Mr. Hunter.

Others again have contended, that the condition of the bloodvessels does not depend on the part of the stomach acted on, but is solely regulated by the stage of solution at which we inspect the parts. If, say they, we examine the stomach, when its inner coat has chiefly been acted on, the vessels will be seen ramifying on those coats which are only partially affected, and blood may be squeezed from the ends of the vessels; but if a complete perforation have been made, we can neither see vessels ramifying on the dissolved part, nor force blood from their ends. Now, I believe it will be found that neither of those afford a satisfactory explanation of the point in question; but as these opinions are contained in papers published regarding a late case which occurred in Liverpool, into a discussion of the merits of which I do not wish to enter, I forbear to make any further comments on them.

pearance of the vessels wanting, the condition of the individuals at the time of death was very different from what it had been in Mr. Hunter's cases. In the former, the people were worn out by debilitating diseases; they were emaciated and anasarcous, with hardly any blood in their vessels, and not a great deal even in their hearts. I was not therefore surprised, when I found no blood in the gastric vessels of these subjects. It would indeed have been more astonishing had these been found containing blood, while the vessels of all the neighbouring viscera were empty. I have with confidence ascribed the solution of the coats of the stomach, in these cases, to the gastric juice, and in proof shall hereafter mention the effect produced on those parts in contact with the hole in the stomach. The facts which have been mentioned show, I think, that digestion of the coats of the stomach may take place under two very different conditions of the body. They prove, that although such solution is most frequent in those who have been suddenly deprived of life when in full health, that it is not confined to those alone, but does, under certain circumstances, with which perhaps we may never become acquainted, occur in those who have died from lingering and debilitating diseases. This last position I bring forward supported by complete proof, without which I would not have ventured to make it public, since it is so opposite to the opinion adopted by many whose authority on such subjects is deservedly high. Perhaps this idea may have originally arisen from the circumstance of Mr. Hunter not having been so explicit as we could have wished he had been. Be this, however, as it may, the opinion is at present so prevalent, that some practitioners of eminent abilities who have lately written on this subject, have asserted, that they can hardly conceive it possible that the stomach can be dissolved by the gastric juice in a person in a state of debility at the time of death. Nor is this the only mistaken notion which seems to be at present entertained on this subject. Mr. Hunter taught, that digestion of the stomach after death was occasioned by that portion of the gastric juice contained in the cavity of the stomach; consequently it followed, as a fair inference from this doctrine, that the coats of this viscus will only be acted on at that part on which the contents of the stomach rested. In Mr. Hunter's

cases, the great end of the stomach, which in the supine position of the body is the most depending part of this viscus, was found to be chiefly affected; a fact which tended to corroborate and support his opinion, and to render extremely probable his conjecture. But this assertion, coming from Mr. Hunter, had a bad effect; for when practitioners met with instances of solution of other parts of the stomach than the great end, they applied to his essay for information on this point; but finding nothing there to render it even probable that such solution might be effected by the gastric juice, other causes were sought for. Poison has been called in to account for the appearance; and thus the life of the person supposed to have administered the deleterious substance might, it is evident, be brought into danger. As Mr. Hunter was seldom in the habit of drawing inferences from an insufficient number of facts, any doctrine broached by him must be rejected with extreme reluctance, and not until we have satisfactory proof of its fallacy. In the present case, Mr. Hunter has surely deviated from his usual caution, in forming the opinion that the great end of the stomach will be the only part of the stomach acted on. The following history will show that he had. About ten months ago I had occasion, two days after death, to open the body of a very emaciated and anasarcous young girl, who had died from scrofulous enlargement of the mesenteric glands. On raising the coverings of the abdomen, the stomach, which was empty, presented itself to view, with its front dissolved. The aperture was of an oblong shape, about two inches in its long diameter, and an inch in its short, with tender, flocculent, and pulpy edges. This I demonstrated to the pupils attending my class; and I especially called their attention to the fact, that the liver, which was in contact with the hole, had no impression made on it. Having proceeded thus far, I placed all the parts as they had been, stitched up the abdomen, and laid the body aside in a cold situation for two days. Then I opened it again, in presence of the same gentlemen, and we found that now the liver, where it lay over the dissolved part of the stomach, was pulpy; its peritoneal coat was completely dissolved, and its substance was tender to a considerable depth. At this time the other parts of the liver were equally solid as before; and as yet every

part of the subject was free from putrefaction. The posterior face of the stomach, opposite to the hole, was dissolved, all except the peritoneal coat; at least the internal coats were rendered pulpy and glutinous. The peritoneal covering had become spongy and more transparent than it ought to have been. There was no blood contained in either the vessels of the stomach, or other abdominal viscera. The appearance of the dissolved part of the stomach, and the digestion of that part of the liver which was over the hole, will be sufficient to prove that the solution, in this case, was really produced by the gastric juice. This being certain, I may be permitted to mention, that the situation of the aperture was different from what it had been in any of Mr. Hunter's cases. It was seated on the fore part of the stomach, about an inch distant from the pylorus, and midway between the smaller and greater curvatures of this viscus, at a part of the stomach with which the gastric juice could not have come into contact, as the body had constantly been in the supine posture.

The same fact I have seen exemplified in other two subjects; but the particulars of these cases I do not consider it necessary to detail, after the full account I have given above.

If, then, the stomach was not acted on by fluid contained in its cavity, how came it to be dissolved? To me it appears, that we cannot, with propriety, ascribe the digestion of the stomach, in every case, to the gastric juice which has been poured into the cavity of that viscus. We are more properly to refer it, in some instances, to the action of the gastric fluid retained in the vessels which had secreted it. If this be admitted to be a correct explanation of the fact, that the fore part of the stomach is sometimes digested by the gastric juice, we shall cease to have any difficulty in accounting for the dissolution of other parts of this viscus, besides the large end. We shall learn, that the part acted on must vary, according to the place of the stomach where the gastric juice is retained in the apparatus which secreted it, and thus we shall be enabled to explain some cases, which, at present, seem to be in opposition to the observations of Mr. Hunter.

I cannot finish these remarks, without calling the attention to one point noticed in the history of the case: I allude to the state

of the neighbouring viscera, at the opening of the body, two days after death. At that time, none of them were in any degree affected; yet it has been seen, that when the subject had been allowed to lie for other two days, that part of the liver, in contact with the dissolved portion of the stomach, was also in a state of solution. At this stage, the coats of the stomach opposite to the hole were likewise acted on. It is, therefore, to be remembered, in judicial investigations into the cause of dissolution of the coats of this viscus, that the appearances will vary, according to the period after death at which the body is examined.

The opinion, of the digestion of the stomach being produced by that portion of the gastric juice contained in the cavity of this viscus, will be found to be enforced by Mr. Everard Home, in his paper "on the structure and uses of the spleen." Mr. Home supposes, that the human stomach is divided by a muscular contraction, into a cardiac and pyloric portion; and he further believes, that, in the first, the food is acted on by the gastric juice. It is there that it is reduced to the pultaceous mass, which afterward is propelled into the pyloric cyst, where the chyle is separated from it. "That the food is dissolved in the cardiac portion of the human stomach, is proved, by that part *only* being found digested after death; the instances of which are sufficiently numerous to require no addition being made to them. This could not take place, unless the solvent liquor was deposited there." I do not pretend to decide on the propriety of Mr. Home's opinion, regarding the part of the stomach in which the food is digested, but I think the cases which I have mentioned go a considerable way to disprove it. But here, I must point out the fallacy of one point of his statement. Have we not seen that the fore part of the pyloric end of the stomach has actually been found digested after death? and having seen this, are we not, equally with Mr. Home, authorized to infer, that the food is alike digested in the pyloric, as in the cardiac portion of this viscus. It may be said, that as the stomach was not, in these cases where its pyloric end was dissolved, divided into two cavities, the gastric juice, which was originally deposited in the cardiac portion, might, in moving the body, have passed into the pyloric part, and thus dissolved its coats. The

case which I have detailed at length will refute this idea. In it, the solution took place at a part of the stomach where it could not have occurred, had it been occasioned by fluid lodged in its cavity. While I thus object to one part of Mr. Home's paper, it is but justice to add, that I often have found the stomach, as he describes, divided by a muscular contraction into cardiac and pyloric cysts.

It may, perhaps, be expected, that I should now assign a reason, why the solution takes place in some subjects and not in others. I am not ignorant of the explanation which Dr. Adams has given of this fact, yet, I know so little concerning this part of the subject, that I feel myself neither warranted to adopt or reject his explanation of it. I do not know whether, as Dr. Adams believes, it depends on the suddenness of actual death, as put in opposition to apparent death, or, whether it arises from some alteration in the quality of the gastric juice. At all events, I consider it sufficient for my purpose, to have pointed out the facts which I have detailed, regarding the digestion of the stomach. I leave it therefore to others, to explain the immediate circumstances, rendering the stomach liable to be dissolved.

As in some degree connected with the accounts given of the digestion of the stomach after death, I may mention, that, in four instances, I have found every part of the alimentary canal, from the cardiac orifice of the stomach, down to the beginning of the rectum, dissolved into a pulpy glutinous mass, transparent, and bearing some resemblance to thick starch. Not a single point of either the stomach or intestinal tube, but was so much acted on, that it tore whenever it was even gently touched. The other viscera were not acted on. The subjects were young children, fat, and free from putrefaction, and in whose bodies I could distinguish nothing satisfactory to account for death. In all of them, the abdomen, when opened, emitted a sour smell. As I never met with an instance of this uniform solution of the bowels in a body, with the history of which I had been acquainted previous to death, I do not wish to enter more fully, for the present, into the history of the cases which I have examined. I may, however, add, that I never observed an occurrence of this kind except during the summer months.

Glasgow, January 1, 1810.

SELECTED REVIEWS.

An Essay on Hydrocephalus Acutus; or, Dropsy of the Brain. By John Cheyne, M. D. Edin. 1808. 8*vo. pp.* 218.

From the Edinburgh Medical and Surgical Journal for 1808.

ALTHOUGH the more prominent and characteristic features of hydrocephalus acutus have been fully and accurately described by former writers, the history of the disease still remained, in many respects, imperfect. Its antecedents, and even the symptoms which often mark its insidious and gradual approach, and earlier progress, had not been well observed. The occasional causes of the disease have been mistaken, or overrated, and very false notions of its pathology have been entertained. Hence the disease has been considered either as a dropsy, or as a phrenitic affection of the brain; and the method of treatment has been consequently vacillating and uncertain, and every one knows but too well how unsuccessful. The object of the essay before us is to supply some of these defects; and we have no hesitation in saying, that Dr. Cheyne has added considerably to the facts which serve to complete the history of this interesting and fatal disease. Some of his pathological observations also are important; but the general view of the pathology of this disease seems still embarrassed and imperfect, and the treatment of the disease, once formed, still where it was. But, as is well observed by the ingenious author, "we are only in the stage of collection, and not advanced, as yet, to that of theory." It is, then, on the very excellent history of the disease, on the numerous and well related clinical cases, and the results inferred from these, that the merits of this performance must rest. But we would more particularly direct our readers' attention to the various modes of attack which distinguish the incipient forms of hydrocephalus, and to the connexion between the hydrocephalic symptoms, and the disordered state of the hepatic and chylopoietic viscera. Of the incipient forms of hydrocephalus acutus, Dr. Cheyne has distinguished three.—In the first, before any characteristic symptoms of the disease appear,

—"The child, for some days, or even weeks, has complained of pains in his head or belly, while, at the same time, he has been slightly feverish, dull, ill complexioned, without appetite, or, perhaps, with an increased appetite, and with considerable disorder of all the functions of the abdominal viscera. These complaints arise gradually, but are seldom alarming; and the child's friends are not awakened to a sense of his danger until, advancing a step farther, the commencement of a peculiar disease has more distinctly shown itself. The dullness and severe pains in his head are now accompanied, perhaps, upon getting up in the morning, or after he has begun to stir about, with vomiting. Yet even this symptom is often disregarded until the second or third day of its recurrence, and the disease has made considerable progress before the illness of the patient is suspected to arise from a disordered condition of the brain. When the attention is more particularly excited by these symptoms, the headache (chiefly in the forehead) will be found to return at shorter intervals. The child often affectingly complains of his head. He sighs frequently, is dull, his head requires to be supported; he complains of weariness in his eyes; the pupils sometimes appear unusually contracted, and he has an aversion to light. His tongue is white, and his belly generally costive; the stools are at first clayey; as the disease advances, they become of a gelatinous consistence, dark green, of a sickly smell, sometimes as dark as tar. The pulse becomes quick, and, at particular times of the day, these symptoms are attended with febrile heat and irritability, and the child complains not only of headache, but of pains in different parts of the body, sometimes astonishingly acute. At one time he complains of pains in his limbs, at another, of pains in his breast, or in the nape of the neck, very often in his bowels; and before the anxiety of his friends can make any preparations to relieve him, the pain is gone, or fled to some other part; at another time, he for a long time lies on his mother's knee, restless and whining, as from dull rheumatic pains. These disorders cannot continue without impairing the child's strength; and accordingly, in ten days or a fortnight, the period usually occupied by the first stage of this attack, he is altered in appearance; in his manner he has be-

come feverish and undecided, his hand tremulous, and his gait tottering."

"In the second form of the attack, the disease runs a more rapid course. This does not occur so frequently, yet it is well intitled to the attention of every one wishing to gain a clear conception of hydrocephalus. After the child has been drooping for a short time, which, although it sometimes escapes observation, is generally recollected, there is a sudden change to a fever, attended, even from the first, with a great degree of pyrexia, with frequent, but short and irregular remissions, flushing, severe headache, tenderness all over the abdomen, and increased sensibility, with sometimes brilliancy of the eyes. It is said to be often difficult immediately to distinguish hydrocephalus from fever; and this is the form of the disease in which there is the greatest resemblance between the two diseases; but we are led to suspect some deeply seated evil, from the frantic screams and complaints of the head and belly, alternately with stupor, or rather lowness; and we are struck with the irritability of the stomach, in a degree beyond what we find in fevers of this country, retching and vomiting being often brought on by a change of posture, certainly by every attempt to sit up in bed; and with the disordered state of the bowels which attends this irritability of the stomach. And when, at any time, the child has a little respite from the violence of these symptoms, we find our suspicion confirmed by his look; for in this disease, when the features do not express pain or terror, there is not unfrequently an expression, which it has in common with some other diseases of the brain, of dejection bordering upon insensibility, which is quite insupportable to those who are interested in his recovery."

The third mode of attack our author has observed less frequently than the first, but more so than the second.

"It may be considered as an instance of that conversion of diseases, which arises either from the excess or combination of the symptoms of the original complaint, operating upon a habit favourable to the new disorder, or perhaps from the state of the habit produced by the primary giving rise to the consequential disease. When hydrocephalus arises after an imperfect state of health, as where there had been a scrofulous action

which has abated; or where from predisposition and the anomalism of the symptoms, such action is looked for; or where the child has had some epidemic disease formerly, (perhaps many months before) from which he has not perfectly recovered, or regained sound health; the attack is sometimes made with all the violence which I have described as distinguishing the second form. When again the attack comes on as the sequel of an acute disease or fever, hooping-cough, perhaps dentition, or during some actually existing scrofulous disease, then the child almost imperceptibly slips into hydrocephalus; there are searcely any of the acute symptoms; and the palsies or convulsions are the first indication of the new disorder."

We have thought these descriptions of the modes of attack of so much importance, that we have given them in the author's words. It is by attending to these, that the disease can be detected in its earlier stages, and when it is most under the control of art. Our author then goes on to complete the general history of the disease. The three stages marked by Dr. White, as distinguished by the changes of the pulse, he thinks may be more usefully retained under some change of character. The 1st, as the stage of increased sensibility; the 2d, the stage of decreased sensibility; the 3d, the paralytic or convulsive stage.

"In the first stage, every stimulus produces a sensation more than proportioned to its common effects. There is great aversion to light, and to sounds; there is watching, sickness, pain, a quick pulse. In the second stage, the child is not easily roused, his pupil is dilated, his pulse slow, he is lethargic, with often an obstinately costive belly. In the third stage, which perhaps may be considered as a continuation of the second, there is squinting, rolling of the head, raving, stupor, convulsions, with a rapid thready pulse."

Amongst the causes of hydrocephalus, there is none perhaps of greater moment than that which depends upon a deranged state of the abdominal viscera, upon a morbid state especially of the liver and alimentary canal. The subject is accordingly investigated with great minuteness, and many facts and observations are brought forward in proof of the intimate connexion between these disorders of the primæ viæ and this dangerous

disease of the head; such are the well known association between the actions of these organs; the great disorder observed in the chylopoietic viscera, often for weeks before the appearance of characteristic hydrocephalic symptoms; the removal of well marked symptoms of hydrocephalus by the use of purgatives, and other remedies directed to the bowels; the tenderness of the region of the liver, remarked by the author in children under hydrocephalus; the remains of great inflammatory action in this viscus, observed by him after death; and the very peculiar stools which belong to the disease, indicating a changed state of the biliary secretion.

The appearances on dissection after death from hydrocephalus, are thus concisely stated;

"We generally find within the cranium the veins, particularly those of the membranes on the surface of the brain and lining of the ventricles, gorged with dark blood; sometimes considerable adhesions between the thickening of the membranes, and minute and florid vessels upon the pia mater. The ventricles we find to contain from two to six ounces of limpid serum; also fluid in a small quantity under the tunica arachnoides, both above and at the base of the brain. The substance of the brain is generally soft and blanched, fimbriated, and particularly soft where it forms the ventricles. The substance of the fornix is often like a soft curd. In the abdomen, I have found the intestines inflamed and constricted from spasm, and the surface of the liver of a bright red colour, abounding in minute vessels; and sometimes extensively adhering to the peritoneum. In several dissections, I have found the surface of the liver studded with small white tubercles, not larger than a grain of mustard. The glands of the mesentery are often diseased, as is evinced by their enlargement, and the caseous depositions which we find in the substance of the glands."

In investigating the pathology of hydrocephalus, the opinions of Whyte, Quin, and Rush, are stated and canvassed with much judgment and candour.

The effusion of water, whence the disease has derived its name, is evidently the effect of the disease, and not its cause. In some cases no effusion has been detected; and it is probable

that in every case the effusion does not take place, but in the latter stages of the disease.

The action which takes place in hydrocephalus, is shown to be very different from the inflammation of phrenitis; the opinion of Dr. Quin seems to approach nearer the truth; but it is observed by Dr. Cheyne, that there is some change prior to the morbid accumulation of blood, "there must be an active before there is a passive state." He is inclined to think that the morbid action in the brain is specific; that excitement first takes place, then increased vascular action, congestion, and effusion.

"That in this disease there is produced a venous congestion, in addition to, and probably arising from the increased arterial action: that the effusion of serous fluid arises from this venous congestion: that this effusion has a tendency to counteract the baleful effects of the increased action, and to retard the fatal termination of the disease; of course, that the effusion into the ventricles is not the cause of the violent symptoms; and that the increased arterial action, though perhaps varied, does not cease when the congestion and effusion have taken place."

The stage of torpor he supposes to commence with the congestion of the venous system. Effusion is not considered as the cause of death, because large effusions of serous fluid have long subsisted with the continuance of life, as in chronic hydrocephalus.

"It is the morbid action of the bloodvessels wasting the brain, and unfitting it for its functions, which destroys the child; and I think it no rash opinion that death would occur earlier in the disease, did not the fluid exude, and thus continue to the brain the necessary degree of support from within, which would have been lost by the wasting of the organ."

This, however, we are disposed to consider merely as a bold and ingenious hypothesis. The author has at least shown, that the presence of water in the ventricles is not necessarily fatal to life; though in some of the instances it should be remarked, that the pressure from the fluid may have been obviated by the separation of the bones, and enlargement of the head, as in chronic hydrocephalus; and in other instances it is impossible to say how long the water had been effused before the cessation

of life, or if in some it had not been the immediate cause of death. The stage of excitement and increased sensibility, has been attributed by Dr. Cheyne to the increased vascular action; the stage of torpor to the congestion in the veins; and is it not highly probable, that the paralytic and convulsive stage, which ushers in the death of the patient, marks the effusion into the ventricles?

Having recapitulated the pathological facts unconnected with hypothesis, and observed that we are at present ignorant of any way of subduing hydrocephalus, without the substitution of another constitutional irritation, Dr. Cheyne proceeds to the treatment of the disease. The indications of cure laid down by him are,

—"To remove from the constitution every irritation which either may have given rise to, or may have assisted, by morbid sympathy, in prolonging the diseased action in the brain;—to subdue the hydrocephalic action, 1st, by diminishing the increased activity of the circulation within the head; 2dly, by substituting a new action;—to alleviate pain and sickness, if ever urgent; to accept every assistance offered by counter-irritation; to support and renew the strength; to support it more particularly under any critical discharge."

The connexion which is shown to subsist between irritation in the alimentary canal, and the hydrocephalic excitement, claims, according to this view, a very early attention. Strong cathartic medicines are often required, and in most cases the very best effects may be expected from a course of purgatives.

The practice of Dr. Hamilton, and the great success which has attended the use of purgatives, are already known to the public. The experience of our author confirms the utility of this treatment. His plan is, when called early, to recommend

—"the exhibition of the largest dose which can be safely prescribed, of some powerfully cathartic medicine, two, three, or four times a-day; and this continued for several days, or until natural stools are procured. The advantage of keeping the intestinal canal under the continued influence of a stimulus, I have, in various instances, found to be so great, that I am induced to repeat the declaration of my belief, that the happiest result may be expected from this measure."

The increased activity of the circulation within the head, is to be diminished by local or general bloodletting, according to the circumstances of the case; and the abstraction of blood, it is remarked, is useful on another account, to render the constitution susceptible of the powers of mercury.

From blisters he has frequently observed a remarkable mitigation of the symptoms. However, so soon as the existence of the disease becomes probable, no more time should be lost than is necessary for subduing the disorder in the bowels; we must then commence the mercurial course, which, it must be allowed, has cured hydrocephalus even when far advanced. The object of the mercurial course, is to substitute a new action or counter-irritation, by which the hydrocephalic action shall be suspended or subdued: examples of which have come under our author's observation, and under that of other physicians. Digitalis is a remedy of considerable promise in this disease, from the command which it has over the circulating and absorbent systems. The author's experience of this medicine in hydrocephalus, has not been great; but he has observed it to produce a great, and in two cases he conceives a salutary, influence.

A valuable collection of cases is annexed to this essay. From these cases the results and doctrines of the text have been drawn. They are of themselves important and instructive. "They were invariably drawn up in the bed-chamber of the patient, and present a faithful account of every symptom and circumstance that attended the disease."

Practical Observations in Surgery, illustrated by Cases. The second Edition, corrected and enlarged; with additional plates.

BY WILLIAM HEY, F. R. S.

Member of the Royal College of Surgeons in London; Honorary Member of the Royal Medical Society of Edinburgh, and of the Literary and Philosophical Society of Manchester; and Senior Surgeon of the General Infirmary of Leeds. 8vo. pp. 578. Price 13s. London. Cadell and Davies. 1810.

From the London Medical Review for 1810.

THIS is a new and much enlarged edition of one of the most valuable contributions that has ever been made to surgery. So much knowledge without pretension, improved by observation so well conducted and matured, are equally calculated to arrest the public confidence and to maintain it by just decisions.

With exemplary candour, and a degree of zeal exceedingly rare, at least among men of the author's years, Mr. Hey was no sooner apprised of the incorrectness of some terms in his original description of the anatomy of femoral hernia, than he determined to examine for himself the modern illustrations of this important subject. With this view he visited the schools of the metropolis, and with all the ardour of a youth entering on his studies, attended the lectures and examined the preparations of Mr. Astley Cooper. Such an undertaking however, was only accomplished at the imminent risk of his life. In the present edition Mr. Hey has in consequence amplified and illustrated by plates, with perfect accuracy, the anatomy of femoral hernia. In the preface he thus acknowledges and very satisfactorily explains his former misconceptions.

" When I first committed my papers to the press, the public had not been favoured with that elaborate and excellent work of Mr. Astley Cooper on Hernia. I had, indeed, read the treatise on femoral hernia by Don Antonio de Gimbernat; but had not profited by it as I might have done. For not understanding clearly, at my first perusal, his description of the posterior projection of the aponeurosis of the external oblique muscle of the abdomen, I incautiously laid the work aside;

determining, however, to seize such opportunities as might offer of discovering the cause of stricture in femoral hernia.

"On such occasions, I pursued my examination in the following manner. Having laid bare that part of the fascia lata of the thigh, which covers the great femoral vessels, where they descend below Poupart's ligament; I opened the abdomen, and removed the peritoneum, together with that fatty membrane which lies at the entrance of the sheath of those vessels. I then dissected out the lymphatic glands and adipose membrane, which remained in the sheath on the inner side of the femoral vein. Having cleaned these parts, I introduced my finger into the sheath; and, carrying it downwards on the inner side of the vein, till it appeared below what is now called the lunated or falciform process of the fascia lata, I took notice where the stricture upon my finger was the greatest. In doing this I found the anterior edge of the thin projection of the aponeurosis of the external oblique muscle, to coincide or be continued with the falciform edge of the fascia lata. This part I called the femoral ligament. My idea, however, of the anatomy of these parts was not clear when I first adopted that term; and, consequently, my description of them was obscure; though I am not aware that it led to any practical error."

Besides the great additions to the chapter on the strangulated hernia, and other considerable alterations in various parts of the work, the author has annexed to this edition three short chapters on the following subjects. On the hydrocele of the spermatic chord—a case of lithotomy in the female—a case of tumor on the nose. An appendix to the volume contains a case from Mr. A. Cooper of strangulated femoral hernia with mortified intestine.

We shall offer a very few remarks on some points of the work. The convex-edged saw which among us has pretty generally obtained the appellation of Hey's saw, deserves in our opinion, all the encomium which has been passed upon it. The trephine is an instrument which is often quite inapplicable, and has only kept its ground for want of a better; this is now supplied.

In his chapter on the cataract Mr. H. exposes the inaccuracy of the idea conveyed by some writers when they speak of

depressing the crystalline in the posterior chamber. This is however merely a difference in the application of the term posterior chamber, between writers who use it; their anatomy, it cannot be questioned, is the same; their nomenclature here as in innumerable instances at variance. Winslow's description is for obvious reasons the correct one. There exists no other chamber posterior to the iris than that occupied by the aqueous humor. The operation of depressing the cataract is that which Mr. H. is in the habit of performing. His instrument differs from that generally used in its length, being only seven-eighths of an inch; in its shape, being elliptical instead of pointed at its extremity, and from the part where it ceases to be flat, increasing gradually in diameter towards the handle. This construction he considers to be attended with decided advantages; but they appear to us to be founded upon a comparison with very ill constructed needles of the spear shape. We allow that a short needle is the best, but one needle may be as short as another; that increasing in diameter towards the handle is advantageous, but such a construction is common to all the needles which we have seen used. "This needle has no projecting edges, nor any projecting point," says Mr. H. These *we* reckon among its disadvantages, for if it had projecting edges, the operator has no business to wound the ciliary processes or iris, and if he wounds them with one instrument he will with another; but angular edges will do more execution than parallel ones, and if it had a projecting point it would enter the globe of the eye with as much facility and as little pressure as the spear-shaped needle, which we can say from experience it *does not*.

Repeated efforts are stated to be more successful than those which are long continued, to depress the firm cataract, but the soft cataract Mr. H. finds to be the more frequent. "In this state of the disease, I do nothing more," says our author, "than break down the texture of the cataract, and endeavour to puncture or tear off a portion of the capsule, that the aqueous humor may flow in upon the broken cataract. In doing this it is common to see some fragments of the cataract fall through the pupil into the anterior chamber of the eye. I am always glad to see this take place, as I then know that there is a passage

opened for the admission of the aqueous humor, and that those opake fragments which have passed through the pupil will soon disappear." Mr. Hey inculcates great caution in the conduct of the operation, and very strongly deprecates an opposite mode of procedure. The repetition of the operation he considers to be no discredit to it, nor any discouragement to the patient if he be properly made aware of its necessity. He has succeeded in destroying the strict adhesion of the iris to the capsule, by repeating the operation in some instances even eleven or twelve times. In the course of this chapter many observations occur which are clearly the result of extensive practical experience. We expect ere long to have occasion to renew this subject in detail, and shall therefore defer a critical inquiry into the merits of the several operations practised for the cataract.

The anatomy of inguinal and femoral hernia is described with much conciseness and perspicuity. Mr. H. comments usefully on the division of the stricture in the latter disease, and considers that some obscurity has been occasioned by an ambiguous use of terms. *Upwards*, for example, in the supine posture, is *horizontal* in relation to the erect posture. This is an instance in point, of the improper use of common terms relating to position and aspect. "Supposing," says Mr. H. "that the obturator artery passed round the neck of the sac, but at such a distance from the ring that the button of the bubonocele knife could pass freely between it (the artery) and the stricture; it is evident that in making the incision *upwards*, as it is called, the knife would have made no greater approach towards the artery at the end than at the beginning of the incision." On the treatment of gangrene of the intestine, Mr. H. refers to other writers for information. "I have seen several such cases, but the termination of them in general was fatal." After relating two cases of recovery, he conjectures that many recoveries may have been owing to the head of the colon being the sphacelated portion.* This suggestion may be confirmed by the circumstances of some few cases, but it is quite inadequate as a general explanation.

* Vid. Med. Obser. and Inq. Vol. III. Art. 8.

We are presented with some instructive histories of the several modes of treating adhering and mortified omentum. It is by no means yet decided whether reduction, excision, or leaving the omentum in the wound when much altered by disease, is the preferable practice. Reducing an adhering portion of epiplocele and retrenching it have both proved fatal in Mr. H.'s practice. To the ligature he feels all the aversion expressed by the late Mr. Pott, having been a witness of its destructive effect in the very case upon which Mr. Pott's objections to it were founded. On the other hand, a portion of omentum of several ounces weight, he has left in the wound after the removal of the stricture, and after a third of it had sloughed, removed the remaining two thirds to the level of the parietes by the application of a ligature. By frequently renewing this application he succeeded in removing a portion weighing five ounces and five drams avoirdupois, in seventeen days, and in six weeks the wound was nearly cicatrized.*

The result of Mr. Hey's practice has convinced him that large wounds of the omentum are dangerous if the bleeding vessels are not tied. "I shall never again cut off any large portion of omentum without applying a ligature to every bleeding vessel before I permit the remainder of the omentum to retire into the abdomen." But he confesses that with this precaution he has little apprehension of ill consequences, and there are cases in which its excision must be beneficial. Mr. Pott who, though in the main a very good surgeon, was a very great dogmatist, has spoken with a degree of temerity on this subject, which nothing but the candor of a weak man or the infatuation of a prejudiced one will excuse.

The knee joint is subject to some internal derangement from slight accidents, as twists, sprains, and blows. This is probably a partial dislocation of the convex articular surfaces of the femur, from the semilunar cartilages of the tibia. Mr. Hey, who first noticed the case, has invariably given instant relief by a complete flexion of the leg while the patient was in the sitting posture, as on the edge of a table. A well adapted laced knee-

* Mr. Hey found the practice of leaving gangrened omentum in the wound to answer well in three cases in which he has tried it. In two the diseased part was cast off on the 7th day; in the third on the 11th.

cap Mr. H. has found of very essential service in the cases of loose cartilaginous substances in the joints. This practice has not uniformly succeeded in removing the lameness and pain, or preventing the return of the disease. We have seen these substances which are as much bony as cartilaginous, removed with considerable difficulty, and very severe symptoms follow the operation.* Very lately however, a case has occurred in which the operation was done in such a way as to prevent symptoms of irritation. This consisted in making the orifices of the skin and of the ligament at a considerable distance apart, by previously stretching the integument. A valvular covering to the internal incision was thus afforded by the skin resuming its situation, so that no more irritation succeeded than if the wound had not extended beyond it.

The successful termination of so many cases of wounds and compound luxations of the joints, convinces us, with the highest respect for the skill of the Leeds Infirmary, that its surgeons have constitutions to work upon which give them a high vantage ground over those of the metropolitan hospitals. The chapters on retention of urine, and on amputation deserve very attentive perusal.

Several distressing cases of *procidentia ani* the author has completely relieved by removing with the knife the flaps of skin and tubercles that lie on the side of the anus, inducing by this means adhesion of the cellular substance to the replaced gut.

We feel somewhat disposed to doubt if Mr. H. has properly named all the cases included under the head "Cancer of the Penis." In eight out of ten, there was a congenital phymosis. Now we believe that this fact has a relation to the origin of the disease, and therefore renders it vastly improbable that it was cancer. We have little doubt that it was so in case 4, but we are rather surprised that it did not occur to Mr. H. that the confinement of the matter secreted by the *glandulæ odoriferæ*, the irritation of surfaces unnaturally opposed, in the venereal act, &c. might account for a fretful ulceration and an unhealthy discharge which in its appearance, not in its nature,

* See the Medical and Surgical intelligence of the present month. [July.]

resembled cancer. It strikes us upon the aspect of the cases, that he was led to consider the disease carcinomatous upon too slight evidence: that he exercised less than his usual deliberation of decision. We may be mistaken in the fact: we judge only from the document before us. We know that the texture of the prepuce and skin of the glans penis is subject to excrescences which fungate and ulcerate, and give the surgeon much trouble. But it is remarkable (if the disease was cancer) that it should only in one of eight cases affect the neighbouring glands; that not one of the eight patients should have any return of the disease; that the ulcerated prepuce and frænum having been extirpated, the wound should cicatrize in a fortnight, and the penis remain sound (case 6); that a mere division of the prepuce should cure the disease, as in all probability it did, since Mr. H. has heard nothing of his patient since the cure, which was several years ago. (Case 7.)

In a case precisely corresponding to Mr. H.'s description, which some little time ago brought a country gentleman to London, the part was condemned to be amputated by a surgeon of the first eminence, which so alarmed the gentleman, yet vigorous in years and constitution, that he consulted another surgeon of equal eminence, who laid bare the penis to the extent of the disease, and cured him by local applications.

We shall conclude these remarks by the extract of Mr. Hey's last case (10) in his own words, which will prove, we think; that the question is not merely of importance in pathology, but in practice, whether this disease of the prepuce, for there it commences, is or is not cancerous. If it is decided in the negative, the following practice is unnecessarily severe: whatever it be, one most important practical inference which we are surprised that our author should have omitted to make, is furnished by these cases; *a congenital phymosis ought never to be suffered to exist beyond the age of puberty.* We can say from a recent performance of the operation merely to remove the inconvenience, that it is attended with perfect relief if the division is of sufficient extent, and is followed by very trifling deformity.

" Jonas Royds, aged 76 years, who had a congenital *phymosis*, about nine months before his admission into the Leeds

Infirmary, (March 16, 1310) perceived an enlargement of the extremity of the penis, especially on one side. The diseased part felt hard, and was prominent. A bloody serum issued frequently from the prepuce, and the discharge of his urine gave him pain. Nothing morbid appeared in the integuments; nor was any ulceration perceptible in the prepuce. In other respects the patient was healthy.

"20th, I amputated the diseased part; and, upon examination, found one half of the prepuce ulcerated internally, and covered with a cancerous excrescence, which extended along the corona glandis, from the frænum to the dorsum penis. The rest of the glans was in a sound state; and the opposite side of the prepuce was but slightly affected."

Mr. Hey has ingeniously contrived to prevent the involuntary discharge of urine after the operation of lithotomy in the female, by filling the vagina with a plug of rolled linen. It answered well in a case which he relates; but we have good hope that the operation may be rendered altogether unnecessary, from the example of perfect success which we have given in our medical and surgical intelligence of the present number. The revival of the practice of dilatation of the *meatus urinarius* for the removal of foreign bodies or calculi from the bladder, is due to Mr. Thomas; and from this gentleman's paper in the Transactions of the Medical and Chirurgical Society, Mr. Cooper, ever anxious for professional improvement, probably borrowed the idea.

It only remains that we should notice the case contributed by the last mentioned gentleman to the work before us: it is one of infinite value. The circumstances are shortly these: "The patient, a woman three months advanced in pregnancy, was reduced to a state of extreme lowness by a strangulation of four days' continuance; the mortified gut was opened so as to admit of a fæcal evacuation at the groin; on the 8th day she passed flatus, on the 8th fæces by the rectum, and continued to do so occasionally and in part for the space of a month, when the fæcal discharge took its natural course uniformly, and the wound in the inguen closed. This happened in the month of *May*, 1808. She was delivered in the October following of a full grown dead child, and some time after miscarried; from

that time she has continued well in every respect, and only wears the truss when exposed to unusual fatigue." The following is Mr. Cooper's conclusion.

"From the foregoing history it appears that the proper treatment of a mortified intestine in strangulated hernia, consists in the two following circumstances:

"1st. In dividing the stricture so as entirely to remove the cause of strangulation; and,

"2dly. In making an opening into the intestine so as to give a free outlet for the discharge of the accumulated fæces in the intestine within the abdomen. If the stricture only is divided, the constipation, hiccough, and vomiting, continue; but if the intestine is opened, the patient in a few hours becomes relieved of those symptoms.

"This case will serve as an answer to a query in the London Medical Review for April 1808. 'It remains for the candid observation of those who have the opportunity to determine, Whether in cases where the patient survives the operation, the removal of gangrenous intestine by the scissars is, or is not, an objectionable practice? *and whether it is giving a fairer chance of recovery to second the efforts of the constitution during the process of separation, while we facilitate by a free opening the evacuation of the alimentary matters?*'"

We shall for the present forbear to comment on this interesting case further than to congratulate ourselves on having suggested the practice to which, as we believe, this woman owes her life.

Nothing indeed can be more opposite to this practice than that which Mr. Cooper has laid down in his work on hernia, viz. the removal of the mortified portion of intestine, and the connexion of the extremities by sutures; and we regard this as an honourable proof that his interest in the advancement of the profession overpowers those little feelings and political calculations, which have too often induced in men of eminence a pertinacious adherence to their published opinions.

The case which occurred in June, is an incontrovertible instance of the practical expediency of the proposal which we suggested in May preceding. (L. M. R. vol. I. p. 131, 132.) This fact Mr. Cooper has handsomely acknowledged, by pub-

lishing it in the form of a reply. If the obvious conclusion should be borne out by future results, it will be a subject of gratification to us, to reflect that the London Medical Review has not lived in vain.

Non ex quovis ligno Mercurius fit.

We cannot close this volume without declaring that in the course of our critical labours, we have never closed one with a stronger conviction of its value, or a higher respect for its author. We recommend it without hesitation or reserve to three classes of persons; first, to writers on practical subjects as a model of useful composition both in substance and in style; secondly, to established surgeons, as a book of reference for many valuable facts in practice; thirdly, to students, as a book which the smallest professional library should contain, and the largest cannot be complete without.

The Principles of Midwifery; including the Diseases of Women and Children. By John Burns, Lecturer on Midwifery, and Member of the Faculty of Physicians and Surgeons, Glasgow, London*, 1809. 8vo. pp. 519.

From the Edinburgh Medical and Surgical Journal for 1810.

WE already possess some excellent treatises on the principles and practice of midwifery; and, as this is a department in which no very important discoveries or improevments have very lately been made, we should have thought it at this time no very easy matter to usher into public, with any fair prospect of success, a new publication on the subject. All, however, that could be expected, in the present state of the science, to give a new interest to the subject, Mr. Burns, equally experienced as a teacher and practitioner, has, by a judicious arrangement, by a faithful exposition of facts and observations, and by a methodical induction of the principles and practice

* An edition of this very valuable work, with notes, has been recently published by Dr. Chapman, Lecturer on Midwifery, Philadelphia.

of the art, from these fundamental truths, contrived to accomplish in the work now before us. It is divided into four books; the first, comprehending nineteen chapters, treats of the structure, functions, and diseases of the pelvis and uterine system, in the unimpregnated state, and during gestation; the second, of parturition; the third, of the puerperal state; and the fourth, of the management and diseases of children. So that every subject, directly or indirectly connected with the practice of the accoucheur, is here brought into one connected view. This, indeed, is the great advantage which Mr. Burns's work must be acknowledged to possess over all others, as a system or class-book. But what we are most disposed to recommend in this volume, is the pathological department, and the descriptions and treatment of the diseases of puerperal women and of children. A more copious, scientific, and judicious account of these diseases, is, perhaps, no where to be met with.

To enumerate some of these, in the order in which they are treated, will convey to our readers some notion of the value of this part of our author's labours.

1st. Diseases of pregnant women:

Febrile state; vomiting; heart-burn; fastidious taste; spasm of stomach and duodenum; costiveness; diarrhœa; piles; affections of the bladder; jaundice; coloured spots; palpitation; syncope; dyspnœa and cough; hæmoptysis and hæmatemesis; headach and convulsions; toothach; salivation; mastodynia; œdema; ascites; redundance of liquor amnii; watery discharge; varicose veins; muscular pains; spasm of ureter; cramp; distention of the abdomen; hernia; despondency; retroversion of uterus; antiversion; rupture of uterus; abortion, and treatment of pregnant women; uterine hemorrhage; false pains.

2d. Of the Puerperal State:

Treatment after delivery; uterine hemorrhage; inversion of the uterus; after pains; suppression of the lochia; retention of part of the placenta; strangury; spasmodic and nervous diseases; milk fever; miliary fever; inflammation of the uterus; peritoneal inflammation; puerperal fever; swelled leg; paralysis; mania, &c.

3d. Diseases of Children:

Congenite; surgical diseases; diseases of dentition; cutane-

ous diseases, viz. strophulus, lichen, intertrigo, crusta lactea; anomalous eruptions; pompholyx; pemphigus; miliaria; prurigo; itch; herpes; itchthyosis; psoriasis; impetigo; pityriasis; porrigo; scabs; boils and pustules; petechiæ; erysipelas and erythema; excoriation; ulceration of gums; erosion of cheek; aphthæ; syphilis; skin-bound; small-pox; cow-pox; chicken-pox; urticaria; measles, and roseola.

Hydrocephalus; convulsions; croup; hooping-cough; catarrh; inflammation of pleura and stomach; vomiting; diarrhœa; costiveness; chorea; marasmus; tabes mesenterica; worms; jaundice; diseased liver; fevers.

A systematic work like this, embracing so great a variety of description, does not admit of any more extended analysis, or criticism; we shall, however, to enable our readers to form some judgment of the manner and style of Mr. Burns, add a very few extracts.

"Chlorosis succeeding to abortion, laborious parturition, or fever, is often attended with symptoms resembling phthisis pulmonalis. In many instances the pulse continues long frequent; there is nocturnal perspiration, considerable emaciation, with cough and pains about the chest, and yet the person is not phthisical, she suffers chiefly from debility; but if great attention be not paid to improve the health, the case may end in consumption; and hence many consumptive women date the commencement of their complaints from an abortion, or from the birth of a child, succeeded by an hemorrhage. In chlorosis, the symptoms are induced, not by previous pulmonic affections, but by some other evident cause of weakness; the pulse, although frequent, is not liable to the same regular exacerbation as in hectic; a full inspiration gives no pain, and little excitement to cough; the person can lie with equal ease on either side; the cough is not increased by motion, nor by going to bed, but is often worst in the morning, and is accompanied with a trifling expectoration of phlegm. It is not short like that excited by tubercles, but comes in fits, and is sometimes convulsive; whilst palpitation, and many hysterical affections, with a timid and desponding mind, accompany the symptoms. The bowels are generally costive, and the person does not digest well.

"In chlorosis attended with symptoms resembling phthisis, it is of considerable utility to administer, occasionally, a gentle emetic, and at the same time, the bowels must be kept open. Myrrh combined with the oxyd of zinc, is, I think, of approved efficacy; and the ammonia given in the form of an emulsion, with oil, very often is effectual in removing the cough. A removal to the country, and the use of moderate exercise on horseback, will contribute greatly to the recovery. The diet ought to be light but nourishing. In many cases, milk agrees well with the patient, but it is not necessary to restrict her from animal food. Pain in the side may be removed by the application of a warm plaster; and if the cough be troublesome, the squill may be used as an expectorant, and an opiate should be given at bed-time. If the skin be permanently hot, or irregularly hot and cold, without weakening perspiration, the tepid bath is of service, or small doses of saline julep may be given. Should the person be of a phthisical habit, and the symptoms increase or continue obstinate, it will be proper to remove her to a mild climate, or the southern part of the island. Emmenagogues are either useless or detrimental." p. 100-1.

After having described the operation of embryulcio, the cases requiring it, and the necessity of breaking down the cranium and separating the bones, in cases of extreme deformity, Mr. Burns continues:

—"We have then only the face and the basis of the skull, and if the pelvis will allow these remains to pass, then the crotchet can be used. I have carefully measured these parts, placed in different ways, and entirely agree with Dr. Hull, a practitioner of great judgment and ability, that the smallest diameter offered, is that which extends from the root of the nose to the chin. For in my experiments, after the frontal bones were completely removed, this did not in general exceed an inch and a half. It is, therefore, of great advantage to convert the case into a face presentation, with the root of the nose directed to the pubes. The size of the crotchet, which ought to be passed over the root of the nose, and fixed on the sphenoid bone, must, however, be added to this measurement. I never have yet been so unfortunate as to meet with what may

be considered as the smallest pelvis admitting of delivery, per vias naturales; but I would conclude, that wherever the pelvis, with the soft parts, measures an inch and three quarters, or if the head be unusually small, an inch and a half, the crotchet may be employed, provided the lateral diameter of the aperture in the pelvis be three inches, or within a fraction of that, perhaps two inches and three quarters, if the head be small or very soft; and the operation will be easy, as we extend the diameter of the pelvis, beyond what may be considered as the minimum." p. 264-5.

The cutaneous diseases of children, so numerous, and often so difficult to discriminate, are very fully described by Mr. Burns, under no fewer than thirty-seven different sections. We select the following excellent account of a very dreadful disease affecting the face and mouth:

—" Another corroding disease begins in the cheek itself, or the lip. It commences with some degree of swelling, which is hard, and firm, and shining. It generally begins on the cheek, which becomes larger than the other; and the upper lip becomes rigid, swollen, and glossy. On some part of the tumefied skin, generally on the cheek, we observe presently a livid spot, which ulcerates and spreads, both laterally and downwards. Being generally situated near the mouth, it soon reaches the gums, and even the tongue partakes of this disease, which is of horrible aspect. We often find a great part of the upper or under lip destroyed, perhaps only a flap or portion of the prolabium left, all the rest being eaten away. The gums are foul, the teeth loose, the tongue thickened, partly destroyed, and lying so close on other diseased parts, that we cannot say what is tongue or what gums, except by the child moving the tongue, and the mouth itself is filled with saliva. The ulcer is foul, shows no granulations, but appears covered with a rough irregular coat of brown lymph. The surrounding parts are somewhat swelled; near the ulcer they are hard and red; farther out on the cheek they are paler, and have more of an œdematous look. These local appearances are accompanied with emaciation and fever, and the child is either restless, or lies moaning in a drowsy state. This disease often proves fatal, sometimes indeed the parts cicatrize, or the patient reco-

vers after an exfoliation of part of the jaw bone. The ulcer is best managed with stimulants, such as diluted muriatic acid, solution of nitrate of silver, camphorated spirits of wine, tincture of opium, &c.; but sometimes it is necessary to give these up for a carrot, or fermenting poultice. The bowels are to be kept open, the strength supported by milk, soups, and wine, and acids, with ripe fruit given liberally. Before ulceration takes place, the best application is camphorated spirit of wine, or we employ friction, with camphorated liniment. A course of gentle laxatives is useful." p. 375-6.

In closing our brief account of this very useful book, we must not overlook the many instructive notes and references to cases and observations, which Mr. Burns has appended to his work, by way of illustrating his text, and enabling his readers to prosecute the study of the subjects to which these refer.

ORIGINAL PAPERS.

Experiments on the Sulphate of Barytes, found at Perkiomen.

BY JAMES CUTBUSH,

Lecturer on Chemistry, Philadelphia.

SOME time since, I made a few experiments on the sulphate of barytes found at Perkiomen, which accompanies several of the ores, in order to ascertain if the component parts corresponded to the analysis of the baroselenite examined by Dr. Withering.

I pulverized a sufficient quantity of the sulphate, which appeared to be crystallized in a tabular form,* and boiled one part of the powder with four of carbonate of potash, in twelve of water.

The water was renewed as it evaporated, and the mixture was agitated frequently.

The whole was now filtered, to separate the insoluble powder, which was repeatedly washed, and the fluid was preserved.

The insoluble portion was transferred into diluted nitric acid; an effervescence immediately ensued, and a solution was effected. The nitric solution was now evaporated, and the saline matter, thus obtained, was exposed in the manner mentioned by Accum, vol. ii. p. 244, to a white heat for some hours. The quantity of the pure barytic earth now obtained,

* The primitive figure of crystallized sulphate of barytes, is a four-sided prism at right angles with its base; which is a parallelogram with angles of 101° 32′, and 78° 28′. These angles almost correspond with those of the primitive crystal of carbonate of lime.

Some of the mineralogical gentlemen of this city have said, that the sulphate of barytes found at Perkiomen, was not crystallized, strictly speaking.

The most common form of the crystal is tabular, with beviled edges.

One fact, however, is certain, that whether the sulphate of barytes found at this place, is amosphous or crystallized, it approaches the state so nearly, that it may be referred to the tabular figure.

was correctly noted. The filtered liquor, which contained the sulphuric acid of the sulphate of barytes, was then saturated with nitric acid, (as it contained a portion of free alkali) and nitrate of barytes was added, until no further precipitate was formed. This precipitate was carefully collected, washed, dried, and weighed. Every one hundred grains indicated, according to the established datum, twenty-three and a half of sulphuric acid.

The small residue left after the action of nitric acid, appeared to be silicious.

The nitric solution contained a portion of iron; for, on adding the tincture of galls to a small quantity, a slight black colour appeared.

By determining the quantity of acid and barytes, obtained from a given quantity of the sulphate, in the manner here stated, the proportion of barytes and sulphuric acid stands thus, as nearly as possible:

Barytes - - -	65
Sulphuric acid - -	28
Iron, silicia, loss - -	7
	100

The theory of this analysis is obvious: the sulphate of barytes is first decomposed by carbonate of potash; the sulphuric acid unites with the potash, which remains in solution, whilst the carbonic acid combines with the barytes, and forms the insoluble powder.

The carbonate of barytes was then treated with nitric acid; carbonic acid was disengaged, and the nitrate of barytes was formed. This was then exposed to an intense heat, to expel the acid, and the barytes remained in a pure state.

The fluid which passed the filter, and which contained the sulphuric acid of the sulphate, was then mixed with a portion of nitric acid, to saturate the uncombined alkali, and precipitated by nitrate of barytes.

The whole of the sulphuric acid was then thrown down, and the precipitate, when collected, &c. gave the proportion of the

acid. By reducing the quantities of the relative ingredients, as given before, the proportion of each per centum was ascertained.

The examination of a specimen of sulphate of barytes, by Dr. Withering, gave 67·2 of barytes, and 32·8 of sulphuric acid.

The Bologna stone, which possesses the phosphorescent property mentioned by chemical writers, although it contains sulphate of barytes, is different in its composition from the *heavy spar* of Perkiomen. In the Ann. de Chem. (1788), the analysis of this stone is given. It is met with in a gray argillaceous marl in Monte Palerno, near Bologna. The component parts are stated as follows:

Sulphate of barytes - -	62
Silex - - - -	16
Alumine - - - -	14·15
Sulphate of lime - - -	6
	98·15

Klaproth (Analytical Essays, vol. ii. page 227) examined a fibrous variety of sulphate of barytes, which was supposed to have been calamine, obtained from New Leiningen, in the Palatinate. He decomposed 300 grains by 600 grains of carbonate of potash, and the carbonate of barytes, thus procured, he dissolved in muriatic acid.

After obtaining the muriate of barytes in a crystallized state, he redissolved it, and added the fluid procured by filtration. As this contained the sulphuric acid of the sulphate, in union with the potash, it precipitated the barytes from the muriatic acid, so that by this process he reproduced the sulphate of barytes. Its weight, after washing and drying, amounted to 297 grains.

From the analysis of the ponderous spar, or sulphate of barytes, presented to us by different analysts, it would appear, that this mineral varies considerably in the proportion of its constituent parts. It is stated by some to contain generally 84 of barytes, and 16 of sulphuric acid, in the hundred.

I have only to remark, however, that so far as barytes is useful either in chemical researches, or in medicine, the sulphate may be decomposed by carbonate of potash, or carbonate of soda, or by decomposing its acid by means of charcoal, and thus afford a means of converting one of the productions of our country to these purposes. The subsequent solution in nitric or muriatic acid, would produce the nitrate or muriate of barytes.

To obtain the muriate from the sulphate of barytes, Mr. Klaproth boiled 16 ounces with 32 ounces of carbonate of potash, and 5lbs. of water, for an hour, in a tin kettle, constantly agitating the mixture, and renewing the water as it evaporated. After filtering it, and washing the carbonate of barytes, to free it of sulphate of potash, and any portion of uncombined alkali, he dissolved the carbonate in pure muriatic acid. The undecomposed sulphate, if any should remain, may be treated again in the same manner.

Van Mons, Westrumb, and Goettling, have severally given us some observations relative to the decomposition of sulphate of barytes, in order to prepare the muriate; but, as the subject would occupy our attention too far in this place, we will only add, that the conversion of the sulphate into the muriate of barytes, for chemical purposes, is a desideratum in our country, in which the native carbonate (barolite) has not been discovered, to my knowledge. Any of the preparations of barytes, may, by thus freeing the earth of sulphuric acid, be very readily formed.

August 15, 1810.

Some Experiments on the Coagulation of the Blood when out of the Body.

BY THOMAS T. HEWSON.

THE blood is fluid while circulating in the vessels of the living animal; but when taken from the body it soon forms a soft tremulous mass. This conversion of the blood from a fluid to a solid state, is termed its coagulation; and must have been familiar to mankind from the earliest ages. The slaughter of animals, the bleedings from wounds inflicted by accident or design, could not fail to establish the fact. The circumstances, however, which favour or prevent the coagulation of the blood seem not to have been, till very lately, the subject of experimental inquiry.

As the temperature of the human body generally surpasses that of the circumambient air, it has been inferred, that the coagulation of the blood, like the congelation of water, depended on the mere abstraction of heat. The analogy of size, and animal jelly, which are fluid in the temperature of our bodies, and become solid when exposed to the medium temperature of the atmosphere, seems to give strength to this opinion. There is, however, this essential difference, that water and jelly assume the solid, or fluid state, according to the degree of heat which is applied; but when once blood has coagulated, it is incapable of being rendered fluid by any additional heat. And farther, it has been very satisfactorily proved by the experiments of Mr. Hewson and Mr. Hunter, that the blood may be actually frozen and converted into ice before it has time to coagulate; and yet on being thawed it coagulates like blood that had never been frozen. Mr. Hewson has likewise shown that the blood coagulates as readily when placed in a heat of 100°, which is equal to the natural temperature of our bodies, as in a heat of 67°, and a heat of 120° produces a more immediate coagulation.

The continual agitation which the blood undergoes during its circulation through the body, has been considered by many as the principal means of maintaining its fluidity; and it has

been urged, that *rest* alone was sufficient to occasion the coagulation of the blood. In support of this opinion, its advocates have adduced the familiar experiment of agitating the blood, when received in a bason, by means of a bunch of rods or small sticks; in which case it is asserted the coagulation will be prevented. If we scrutinize into the circumstances attendant on this experiment, there will be no difficulty in detecting the nature of the deception, and in showing the complete futility of the argument. In fact, the coagulation is complete, and the sticks will be found loaded with solid matter, that part of the blood which is alone capable of spontaneous coagulation, and freed from it, the other parts remain in their natural fluid state.

With the view of acquiring more precise ideas respecting the comparative influence of agitation or rest in the coagulation of the blood, I made the following experiments. About six ounces of blood were received, from a vein in the arm, in a white earthen bowl; the time required for collecting this quantity was three minutes. While the blood was yet flowing from the vein, it was kept constantly agitated with a small bunch of slender sticks. In five minutes the coagulation had evidently commenced, and at the end of five minutes more it appeared to be complete. The whole time occupied in this experiment was thirteen minutes. After the removal of the small clot which adhered to the sticks, the remaining blood was set apart for further examination; and on the next day it still continued fluid, a proof that the whole of the coagulating principle had been removed. In farther confirmation it may be mentioned, that the coagulum, when washed and dried, weighed thirteen grains, equal to the largest proportion of fibrine, I have ever been able to detect in the blood.

Immediately as the first bowl was removed a similar one was employed to receive a like quantity of blood; and here the time occupied in collecting it was likewise three minutes. It was not till the end of ten minutes after it was set by to rest, that there was any appearance of coagulation, and five minutes more were required before it was complete, making in all a period of eighteen minutes.

Hence it appears reasonable to conclude that motion so far from preventing or retarding the coagulation of the blood, actually tends to quicken this process; and consequently that the coagulation of the blood out of the body, cannot be considered as occasioned by rest alone.

It is a common observation that the blood coagulates more readily in broad shallow vessels than in those which are narrow and deep: this has been ascribed to the action of the air. With a view of determining this point Mr. Hewson instituted a number of experiments, the history of which he communicated to the Royal Society in the spring of 1771; and which he afterwards presented to the public in a separate volume. From comparing these experiments* he ventures on the following conclusion: that "the air is a strong coagulant of the blood, and that to this, its coagulation, when taken from the veins, is chiefly owing; and not to cold, nor to rest." Mr. Hunter, on the contrary, after referring to the experiments and observations of Mr. Hewson, asserts,† "that the air has really no more effect than any other extraneous body, in contact with the blood, that is capable of making an impression upon it; for the blood coagulates more readily in a vacuum than in the open air." This has been assumed on the authority of Mr. Hunter, and adopted by Murray in that part of his Elements of Chemistry‡ which contains the history and analysis of the blood. Mr. Allen, formerly a lecturer in Edinburgh, is, I believe, the only one who has publicly expressed any doubts respecting the accuracy of Mr. Hunter's observation. As Mr. Hunter has not described the manner in which this experiment was conducted, we are at a loss to determine whether the vacuum he employed were perfect or not.

It appeared to me that a common injecting syringe might be filled with blood immediately as it flowed from the body, without coming in contact with the air, in which case the agency of the air in the coagulation of the blood would be ascertained.

Having opened the vein in the arm, three ounces of blood were received in a tin vessel. The mouth of a brass syringe

* Hewson's Inquiries on the Blood, p. 21.

† See Hunter on the Blood, Vol. i. page 20, Amer. Edit.

‡ Vol. iv. page 526.

was next applied to the bleeding orifice, a piece of moistened leather fastened to the end of the syringe, and made to embrace the arm, served to exclude the air. The piston was then very gradually raised, and the syringe filled with blood, three ounces, and then corked. About four ounces of blood were taken in an earthen dish. These parcels of blood were placed in a window. At the end of twenty-five minutes the first and last portions were firmly coagulated. Upon removing the cap of the syringe I found about three drams of coagulated matter, having but little admixture of the red globules: the remainder of the blood was fluid, and coagulated on being poured into the dish.

From comparing all that has been stated above, may we not join with Mr. Hewson in concluding that the air is a strong coagulant of the blood, and that to this its coagulation, when taken from the veins, is chiefly owing; and not to cold, nor to rest?

ORIGINAL REVIEW.

An Inaugural Dissertation on the Medical Virtues of the White Oxide of Bismuth, &c. &c.

" —— omnes boni medici ad ea remedia semper confugient, (sive modus " quo prosunt intellectus fuerit necne) quæ certa experientia prodesse " comprobaverit." *Greg. Consp. Med. Theoret.*

By Samuel W. Moore, A. B.—New-York, printed by T. & J. Swords, 1810.

THIS is an interesting and well digested essay. It is written in a style plain and familiar, and the arrangement of its contents is exceedingly perspicuous.

The object of the author is to call the attention of practitioners to a medicine which really seems to us to have been too much neglected. In the execution of this design, he has diligently collected, and brought together within a very narrow compass, much important information concerning its properties, which was previously scattered through a variety of periodical works, and other sources, of no easy access to the generality of readers.

It appears that we are indebted to Dr. Odier of Geneva, for the original introduction of the oxide of bismuth into the practice of physic. So early as the year 1786, he published a paper on the subject in the Journal de Médecine. But prejudices arising against this mineral, which have since been proved to be wholly unfounded, prevented its gaining ground, as a medicine, on the continent of Europe.

The first notice of its being employed in Britain, as far as we have been able to ascertain, is in the Med. & Phys. Jour. for July, 1799. It is mentioned by the editors of that work, as a medicine either neglected or forgotten, " though," say they, " it is stated to be a powerful remedy in spasmodic pain of the stomach and bowels, particularly if it arise from organic debility, or a relaxed and emaciated constitution. One of our correspondents affirms, that he has lately prescribed it in two or three cases of the above nature, not only with apparent, but

with permanent good effect, insomuch, that the cramp of the stomach, which had usually returned once or twice in the month, did not trouble his patients for six and twelve months together."

To Dr. Marcet, a physician of Guy's hospital, the credit is certainly due, of reviving this medicine; and, perhaps, of establishing its reputation. In the year 1801, he read before the London Medical Society, a paper which contained several decisive cases of the utility of the oxide of bismuth, and three years afterwards, on the publication of the paper, he remarks in a note, " I have had since, frequent opportunities, at Guy's hospital, of trying the oxide of bismuth in spasmodic affections of the stomach, and those trials have fully confirmed the opinion which I formerly gave of the utility of this medicine." Confidence in the medicine being thus inspired, it now crept into the practice of many of the distinguished physicians of Britain, some of whom have given cases to the public completely corroborating the preceding statement of its efficacy.

It would be inconsistent with the limits of our work to insert the whole of the evidence which is comprised in the essay before us, respecting the medicinal virtues of the oxide of bismuth. The cases published by Dr. Marcet and Dr. Bardsley, have been well abridged by our author. These abstracts, and the result of some additional cases which came under the care of Dr. Moore himself, or his medical friends, are, indeed, the only portions of this valuable dissertation which we can present to our readers.

Abstract of Dr. Marcet's Cases.

Dr. Marcet prescribed the oxide first in the case of a woman, who, probably from her sedentary occupation, had for two months preceding been afflicted with a very troublesome dyspeptic affection, where no matter, either solid or fluid, was taken into the stomach, without exciting pain at the pit of the stomach, followed by sickness and vomiting; " in short," says Dr. M. " it appeared from her account that only an extremely small portion of the nourishment which she took could be transmitted to the alimentary canal." This patient, for a month,

made use of such tonic medicines as were thought best adapted to her complaint, without deriving from them any sensible benefit; but, on the contrary, she had become thinner than when she had first applied for relief.

" Dr. Marcet having at this time procured a supply of the oxide of bismuth, ordered that she should take five grains of it three times a day, with fifteen grains of the compound powder of gum tragacanth. At the expiration of a week from the time she began its use, she said, " she was almost well," but begged to have " some more of the powders that had produced such remarkably good effects." At the end of another week she declared herself free from complaint; but for fear of a relapse she continued to take the powders a few days longer.

" A girl of seventeen, who " laboured under a chlorosis, the most distressing symptom of which was a violent pain in the region of the stomach," was the second patient to whom Dr. M. gave the oxide. For this patient were ordered pills of aloes and myrrh, from which no effect being observed, in a few days pills of the sulphate of iron with extract of gentian were also given. This course was continued for eight days, during which time the gastrodynia had increased; when twenty grains of the compound powder of bismuth, prepared as in the preceding case, were directed to be taken three times a day, she being at the same time ordered to omit taking any other medicine. On the third day she was much better, though there still continued some uneasiness in her stomach. The compound powder of bismuth was increased to half a drachm three times a day. On the sixth day, on account of the appearance of some other dyspeptic symptoms, she was directed to take, in addition to the oxide, a bitter infusion, with a few drops of aq. ammoniæ, twice a day. On the tenth day, being much better, the dose of the powder was reduced to twenty grains. On the twenty-fourth day from the time she commenced the use of the oxide, she was quite free from complaint, and discontinued taking any medicine.

" In the third and fourth cases in which Dr. Marcet gave this medicine it did not effect cures; but of these cases he observes, " the two instances in which it has failed, the affection, which was at first suspected to depend upon a spasm of the sto-

mach, has since appeared to be of a complicated, and probably of a very different nature."

"A man fifty years of age afforded Dr. M. the fifth case. He complained of having a "most painful sensation in his præcordia, attended with sickness, great giddiness, but not vomiting." This pain, which attacked him every day after dinner, continued four or five hours, and was so violent he could hardly stand; "he had been subject to this complaint for some years past, but for six weeks it had been more constant and severe than ever before." He was directed to take twenty grains of the compound powder of bismuth three times a day. After having taken twenty-four of the powders he was perfectly free from complaint; and six months afterwards his physician heard of his having until that time continued quite well.

"The last case related by Dr. M. was that of a servant girl in his own family. She laboured under amenorrhœa, and was at the same time afflicted with a violent pain in the region of her stomach, attacking her every morning, and sometimes continuing until the afternoon. She made use of sulph. of iron, and other tonics, but the pain continued unabated; she was put upon the use of the compound powder of bismuth, in the dose of twenty grains three times a day, and in three days she was cured of the complaint."

Abstract from Dr. Bardsley's Cases.

The first patient for whom Dr. B. prescribed the oxide of bismuth was a man who had become dyspeptic, probably from the intemperate use of ardent spirits. He was troubled with gastrodynia, frequent eructations, heartburn, griping, and alternate costiveness and diarrhœa. A great variety of tonic medicines were given without affording any permanent relief. He twice became better, though at no time free from complaint, and each succeeding attack was more severe than that which had preceded it. Dr. B. at length having met with Dr. Marcet's paper, prescribed five grains of the oxide, with twenty-five grains of gum tragacanth,* to be taken three times a day. This

* Dr. Marcet's compound powder of bismuth consisted of one part of the white oxide, with three parts of the compound powder of tragacanth. Dr. Bardsley's compound powder contained one part of the oxide, with five parts of gum tragacanth.

patient found considerable relief after having taken nine doses. In nine days from the time he commenced its use he was almost well, except some degree of anorexia, with a slight diarrhœa; to relieve the last symptom gum kino and opium were given with the oxide, and in a short time he was restored to excellent health.

"Dr. Bardsley's second case, he says, "was nearly the counterpart of the foregoing one." The subject of this case, an habitual dram drinker, had been affected for upwards of a year with gastrodynia, which was always increased by eating; he was costive, and suffered much from flatulence. "The contents of his stomach," says Dr. B. "were often so extremely acid, as to excoriate his mouth and lips." After having cleansed his stomach and bowels, he was ordered to take half a drachm of the compound powder, composed, as before stated, of five grains of the oxide, with twenty-five of gum tragacanth, three times a day. On the fifth day he said he was free from sickness and gnawing pains at the stomach, from which he had not suffered since the second day of using the remedy; and he expressed a firm persuasion, that the powders would entirely cure him.

"The remedy was continued, and a few grains of rhubarb and myrrh were also directed to be taken at bed time. This plan was persevered in for about sixteen days, when there was no longer any recurrence of spasm, flatulence, or acidity.

"He took after this, for a few days, a slight preparation of bark and bitters, when he was discharged cured.

"The third case, which afforded an opportunity of using this remedy, terminated fatally. But I conceive by no means to the discredit of the medicine used for its relief, as the disease was in its nature desperate, and probably unsusceptible of being cured by any medicine. This patient had long suffered from dyspepsia, which was supposed to be connected with anomalous gout. He had formerly been addicted to the immoderate use of ardent spirits, but for some time preceding had been temperate. He complained of a fixed tormenting pain at the pit of his stomach, which gradually descended to the naval; the spasms, which generally attacked him after eating, were commonly removed by the contents of the stomach being

thrown up in a very acid state. The bowels were rather costive. Some incurable organic affection of the stomach was suspected to exist; antispasmodics, however, were administered, both combined and separately, without producing any effect.

" Bismuth was then prescribed, with a view of alleviating the spasmodic affection, and preventing the formation of acid in the stomach; and these intentions it completely fulfilled.

" This patient took, as the others had done, half a drachm of the compound powder three times a day, and gradually increased the dose to two scruples; the spasms were subdued, and the general dyspeptic symptoms much relieved in the course of a week. But after continuing for three weeks better, he was suddenly attacked with hepatitis. He recovered from this attack, but only to linger out a few weeks in a hopeless condition.

" It was found, on dissection, that the pylorus was completely indurated and scirrhous, forming a tumor somewhat of a globular form, at least four inches in circumference; the liver also exhibited evident signs of disease. Dr. Bardsley observes of this case, " it may readily be conceived, that nothing beyond a palliation of the more urgent symptoms could have been expected from medicine. The oxide of bismuth answered this purpose, probably, by diminishing the irritability of the stomach, and increasing its tone, and thereby correcting a depraved secretion, and mitigating spasm."

" The subject of Dr. B.'s fourth case had, for more than a year, been troubled with pyrosis, and for a short time had suffered from gastrodynia. His disease was most probably occasioned by his sedentary occupation as a weaver, and his former habits of intemperance: his pain was fixed and dull, not of the spasmodic kind; the quantity of acid discharged from his stomach, in a watery form, was greater than in any of the former cases.

" The stomach having been cleansed by an active emetic, and the bowels emptied by castor oil, he began taking twenty grains of the comp. powder three times a day. Few doses had been taken before the gastrodynia was relieved; the dose was gradually increased to forty grains; and, in the course of a fortnight, he reported himself free from pyrosis, and every other troublesome symptom.

" The next subject of experiment was a house-painter, who had, for many years, been subject to pains in his bowels, resembling colic; and having changed his occupation, was seized every day after dinner with excruciating pain in his stomach, which terminated on its contents being thrown up in an undigested state: he was much reduced in flesh and strength, and had a cadaverous aspect.

" The first passages having been cleansed, half a drachm of the compound powder of bismuth was directed to be taken three times a day; and a blister was at the same time applied to the region of the stomach. On the fourth day he had taken nine of the powders, and had found from them great relief. By the ninth day the spasms were completely removed, and, without the aid of any other medicine, in the course of a fortnight more he was discharged perfectly cured.

" In the last case related by Dr. B. the patient had been afflicted for two years, with a heavy pressing pain in his stomach, attended by a sense of constriction of the œsophagus; he had also acid eructations, the matter ejected being sometimes so corrosive as to blister his palate and lips. He was frequently awakened in the night by a spasmodic affection of the stomach, accompanied by a violent pulsation about the umbilical region. A moderate gratification of his appetite always increased his pain and sickness.

" The doctor confesses in this case his hopes of success were not very sanguine, as he feared the existence of some organic disease of the stomach; the compound powder, however, was prescribed, half a drachm three times a day; a small blister was also directed to be applied to the region of the stomach. On the sixth day of taking the medicine he had already experienced great relief; the stomach could now bear a moderate portion of food without much uneasiness, and no longer ejected its contents in an acid state; the spasms had also abated in frequency and duration. His appetite, however, being still languid, an infusion of bark and colomba was directed to be taken, in addition to the bismuth. On the ninth day his appetite and strength were much invigorated, but he still had some pain at the stomach, where another blister was ordered to be applied.

" By the fifteenth day the tone of the stomach was com-

pletely restored. He continued a few days longer to take the oxide; when, in consequence of a sudden attack of pain in the umbilical region, attended by considerable fever, he was directed to discontinue taking this medicine, to lose blood, and to take a cathartic. This paroxysm continued but a few hours, and the next day he resumed his former plan, by which he was, in a few days, restored to health.

"I feel happy, says Dr. Moore, in having it in my power to relate the following cases, wherein I have had an opportunity of witnessing the effect of bismuth in relieving spasmodic pain, with some other troublesome affections of the stomach; and I have no hesitation in declaring, that it has equalled my most sanguine expectations."*

* *Dr. Moore gives the following Directions for the Preparation of the White Oxide of Bismuth:*

The bismuth to be dissolved should be previously reduced to powder in an iron mortar. Let three parts of nitric acid for one of bismuth be diluted with an equal weight of pure water. To this menstruum, contained in a glass vessel, add the bismuth at intervals, and let it stand till it is all dissolved. Let the clear solution be decanted from the sediment, (a small quantity of which will always be found) and a few ounces of it be poured into a glass vessel capable of containing half as many gallons as there have been measured ounces put in;* the vessel is then to be filled with pure water, when a copious and perfectly white precipitate will be instantaneously formed, giving to the liquid the appearance of milk,

After this has subsided, the clear fluid must be decanted, and fresh water thrown on the precipitate to wash it. This operation must be several times repeated, until no acid taste is discoverable in the decanted water.

This precipitate, which is the pure white oxide of bismuth, should be suffered to dry without heat, or indeed light, for the attraction between bismuth and oxygen is so weak, that if the oxide, while drying, be exposed either to a very moderate artificial heat, or the direct rays of the sun, it parts with a portion of its oxygen, and loses its whiteness.

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* No glazed earthen ware vessel should be employed for this purpose, on account of the lead so frequently used in glazing them. A large wine flask, or demijon, as it is more generally called, capable of containing four or five gallons, will be found a very convenient vessel. Both Fourcroy and Murray observe, that the precipitate is a purer white the more water there has been used in its formation; and I have found that if the quantity of water used be much less than has been directed, the supernatant liquid, after the precipitate has subsided, still contains a considerable quantity of the oxide, capable of being thrown down by the addition of more water.

New-York, Aug. 30, 1810.

"Case 1st. C. a mulatto woman, aged thirty-six, has, for the last seven months, been dyspeptic, during which time I have occasionally visited her, and given her medicines; she is better than she was, but still complains of being troubled every morning, about four o'clock, while still in bed, with sickness, preceded generally by coughing a few times, when she throws up from her stomach a watery fluid, seldom very acid. She assures me, she has not been free from these attacks scarcely for a single day during the last seven months. She has more than once taken emetics, and succeeded their use by infusions of gentian and orange peel, without deriving from them much benefit.

"Although I did not doubt that many cases might be found in which bismuth was calculated to do more good than in the present one; I, nevertheless, felt a desire to give it a trial, and accordingly directed her to take, three times a day, five grains of the oxide, with fifteen of gum arabic and sugar.

"Sept. 5th. She is better, and tells me, she has vomited but twice since she commenced taking the powders. She has taken seventeen doses of the medicine, and has but a single one remaining.

The oxide prepared in this way is purer and whiter than that formed by any other process. I have precipitated it from its solution, both by potash and ammonia, but in neither case was it so pure a white; and, indeed, the administration of the oxide, prepared by means of an alkali, might be attended with danger, in case the bismuth had been alloyed with other metals, which would also be thrown down by the addition of an alkali to the solution.

Whatever reason we may have for thinking the bismuth we make use of impure, (which can scarcely be the case without being discoverable from its appearance) still we have no reason to fear that the oxide will partake of the impurities of the metal which has been employed, if it be prepared in the manner which has been directed; for Nicholson assures us, "the precipitation of the nitrous solution by the addition of water is the criterion by which bismuth is distinguished from all other metals."† And, indeed, if our only object were to separate the bismuth from its alloys, in a state of purity, no better means could be employed to obtain our end than those which have been recommended for preparing the oxide.

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† Nicholson's Chem. Dict.

" 9th. She has very little sickness, but still throws up in the morning a small quantity of fluid from her stomach. I gave her to-day some more of the oxide, with directions to take it as before.

" 12th. Since she recommenced the use of the remedy she has had no return of either sickness or vomiting.

New-York, Sept. 9, 1810.

" Case 2d. Mr. L. aged forty-five, has, for the last three years, been afflicted with cardialgia, attended by extreme flatulence; he has at no time during this period been free from these complaints. He has frequent acid eructations, which he says are also very acrid, occasioning his throat to feel as though it had been scalded; sometimes also, though rarely, he is sick at his stomach, when he vomits up a matter, having the colour of coffee grounds. He has a number of times taken emetics, and, since the commencement of his complaint, has been in the daily habit of taking magnesia, from the use of which, although he has sometimes thought it afforded him temporary relief, it is pretty evident, from the continuance of his disease, that very little permanent advantage has resulted.

" During the last winter and spring Mr. L. had occasional attacks of severe gnawing pains at the pit of his stomach, which, warm external applications would, commonly in the course of fifteen or twenty minutes, relieve. Throughout the summer he has been generally free from these spasmodic pains; but for the last eight or ten days they have attacked him much more severely; usually returning two or three times in a day. These attacks are very violent, occasioning great anxiety, succeeded by insufferable pain, throwing him immediately into a profuse perspiration. The warm applications, from the use of which he had, on former occasions, found benefit, at this time afford him not the slightest relief.

" For the last two months he has been making use of very strong bitters, and I think it probable he has injured his stomach by the large and frequently repeated doses which he has taken. A very sparing use of either fermented or spirituous liquors never fails to increase his cardialgia. His bowels are quite regular, and his pulse feeble.

"Mr. L. at this time sent for my father, who directed him to take, three times a day, five grains of the oxide of bismuth, combined with an equal weight of refined sugar, and twice its weight of starch.

"Sept. 11th. He has had no return of the pain in his stomach since he commenced the use of the oxide, but feels well enough to-day to take a ride into the country.

"Monday morning, Sept. 17th. He still continues free from the spasmodic pain of his stomach; his cardialgia, although not well, is better than it has been hitherto. He has taken none of the bismuth since the morning of the 13th, (Thursday last) when he took the twelfth, and only remaining dose he had, and, being out of town, he had it not in his power to procure another supply until his return to the city to-day.

"Monday evening. He had this afternoon a return of that distressing anxiety which has always hitherto preceded his attacks of pain in the stomach. Fearing he was about to have another attack, and placing full reliance on the oxide, from his experience of its efficacy, he sent immediately for a supply of it, but before the person returned with the medicine he had taken forty drops of laudanum; he, however, took directly a dose of the oxide, and the pain did not, as he had expected, succeed; but, as he had taken laudanum previously, we should not be justified in attributing to the bismuth alone the disappearance of his unpleasant symptoms.

"Wednesday, Sept. 19th. Our patient has not since had any return of the spasms; the cardialgia is infinitely less troublesome. I gave him to-day twenty-four doses of the powder, each containing six grains of the oxide, directing him to take one three times a day.

"Sunday evening, Sept. 23d. Mr. L. is now free from all complaint. His heartburn, which was very troublesome until he began taking the bismuth, does not at present appear, unless it be brought on by the use of wine or brandy; he can, however, drink moderately of West India spirits or gin, without experiencing the least inconvenience from so doing. On Thursday last, after riding twelve or fourteen miles, he began to feel some pain in his stomach, but was immediately relieved on taking his customary dose of the oxide.

New-York, Sept. 24, 1810.

"Case 3d. Mr. B. the subject of the following case, has been for three or four years dyspeptic; during which time he has almost continually suffered from cardialgia and pyrosis, and very frequently severe colic pains. The fluid which he threw up from his stomach was, at times, he said, so extremely acid, that it felt as if it were cutting his throat while passing through it. He has been long in the habit of using alkaline solutions, absorbents, &c. but without effect.

"Mr. B. has, at my request, very obligingly drawn up a statement of his own case, the substance of which I shall give nearly in his own words.

"I have been for several years troubled with acidity on my stomach, a watery fluid running from my mouth, and an unpleasant sensation, or dull pain at my breast, frequent colic pains; sometimes a cough, which was always accompanied by an unpleasant taste: a great heaviness and inclination to sleep, with an aversion to motion. My appetite has always been good, and sometimes, indeed, voracious; but even at these times my stomach would be often disgusted by the first mouthful of food I swallowed. When the heaviness came over me, I felt a sensible diminution of my strength and spirits. I have taken soda, magnesia, chalk, and rhubarb, but have found no relief from them. On the 16th of September I called on Dr. Moore, and obtained from him a few powders, of which I have taken three a day, and have not, for the last five days, felt any symptoms of the complaint.

September 24, 1810."

"Mr. B. took fourteen doses of those powders, containing, altogether, one drachm and ten grains of the oxide. He assures me that he has never felt better than he does at present, and says, that if the powders produce a permanent cure, it will be one of the most happy events of his life.

"Sept. 29th. I saw our patient this day, when he informed me of his having continued thus far perfectly free from all indisposition.

"Dr. Stringham has lately been giving the oxide of bismuth

to a lady labouring under menorrhagia, attended with extreme debility, who has experienced from it astonishing relief.*

" Dr. Post, professor of anatomy and surgery in Columbia college, to whom I, a short time ago, gave some of the oxide, has since informed me of his having prescribed it for the relief of some dyspeptic complaints, and he expresses entire satisfaction with the effect which it produced. He particularly mentioned his having given it to a lady during gestation, who, not only at such times, but generally, suffered much from acid in the stomach. She had derived but little benefit from the medicines ordinarily prescribed in such cases; but was very shortly relieved on taking only a small quantity of the oxide.

* I very much regret that, owing to some particular engagements of Dr. Stringham, it has not been in his power to furnish me, as he had intended, with a particular statement of this case.

INTELLIGENCE,

MEDICAL AND PHILOSOPHICAL.

A REPORT having been lately circulated that Dr. Jenner himself was beginning to entertain some doubts of the efficacy of vaccine inoculation in securing the system against the influence of small-pox, we presume the friends of vaccination will be gratified by the perusal of the following letter addressed to the secretary of the cow-pox institution in Dublin.

Dear Sir,

Your obliging letter of the 2d inst. inclosing the annual report of the cow-pox institution in Dublin, has just reached me. The former letter you allude to, has not yet been delivered. It is with the greatest pleasure I perceived the rapid increase of vaccination in your metropolis, and the uninterrupted success that has attended the practice, at once a proof of the zeal, industry and attention of the medical officers, for which I beg leave to make my most grateful acknowledgments.

And now, sir, a few remarks on the very extraordinary communication you have made to me respecting lady C.—It has been one of the usual devices of the enemies of vaccination, almost from the time of my first making it known, to represent me as having lost my confidence in its prophylactic powers, or at least, that I was wavering on the subject. Can I, who with the aid of my nephews, have vaccinated a number of persons, little short of 30,000, without one single instance of accident, or failure, that ever reached my ears, for a moment entertain such an absurd idea? Or could I have ever thought of inoculating for the small-pox, while I hold that practice in abhorrence, and condemn it both publicly and privately? Believe me, the whole story you relate to me, is an entire fiction, without the faintest shadow of foundation. Never from the commencement of my experiments, to the present hour, have I used a particle of variolous matter, except for the purpose of putting some of those to a test, on whom I made my first trials. For some years past I have relied wholly on the vaccine lymph, for

testing those on whom any material irregularity appeared in the progress of the pustule.

Berkely, Feb. 19, 1809. EDWARD JENNER.

The late British publications announce the discovery of a new and important medicinal property in the rectified oil of turpentine. It is given in doses of from half an ounce to two ounces, fasting, for the removal of tenia, a parasitical genus of animals, which have hitherto frequently baffled all attempts to dislodge them from the alimentary canal. It generally excited an agreeable warmth in the stomach without any degree of sickness or disposition to vomit. The patients however, complained of giddiness, pain in the head, and sometimes of thirst, accompanied by a white tongue, and a warm state of the skin. It would appear that this remedy which seems to act as a poison to the worms, is most effectual when given by itself, and that the distressing symptom of strangury is more likely to occur, if it be employed in small doses, than when exhibited in sufficient quantity to insure its purgative operation. It may therefore be a question to be determined by future experience, whether combining the oil of turpentine, or rather following up its employment with some active cathartic medicine, might not answer an advantageous purpose. There is reason to hope, that this remedy will also prove efficacious in expelling the other worms which infest the human body, while from the quick relief which it gives to horses affected with dry gripes or spasm of the intestines, it may perhaps be worth trying in such cases of colic as do not readily yield to the ordinary modes of treatment.

"Mr. Astley Cooper continues to perform lithotomy by means of the knife alone, with constant success; and we hear that this method has been lately adopted in the Exeter hospital. The prejudice, which this deviation from the accustomed routine will probably encounter, may be diminished by our observing, that surgeons are by no means unanimous in their preference of Hawkins's method. "The cutting gorget," says Richerand, "is employed almost exclusively in England, Italy, Germany, and the north of Europe; the instrument of Frère Cosme, used by individuals in all countries, generally adopted by the

French." (Tom. III. p. 515.) The instrument itself, and the manner of operating with it, are well described in this work. The apparatus, consisting merely of a staff, knife, lithotome caché, and forceps, engages our commendation by its simplicity. The situation and direction of the external incision, as advised by this author, are the best calculated for the purposes of the operation: commencing on the left of the raphé, at about ten lines in front of the anus, it should be continued downwards and backwards, between the anus and the tuberosity of the ischium, for an extent of two or three inches. When the fat and muscles covering the urethra have been divided, the canal laid open in its membranous portion, the lithotome introduced into the bladder, and the staff removed, the former instrument should be so placed, that its cutting edge may be directed obliquely downwards and towards the left, in correspondence with the course of the external incision. It should then be drawn out horizontally, by the right hand grasping the handle, so as to make the blade quit its sheath. In this way an incision is made into the urethra and neck of the bladder, directly opposite to the cut in the integuments, and the forceps, guided by the fore finger of the left hand, introduced for that purpose into the wound, are carried in a straight direction into the cavity of the bladder. Such is the French operation, which, if properly performed, seems to be very well calculated for attaining our objects. The use of the finger in examining the course and size of the incision, and the position of the stone, and afterwards in guiding the forceps; the place of the cut in the integuments; and the course of the opening in the bladder, are circumstances of particular advantage in this plan. The following are the comparative merits of the operation by the lithotome caché, according to the opinion of Richerand. "By cutting as it is drawn out, and not when it enters, a false passage cannot be made, and the neck of the bladder cannot escape division. If it slips from the groove of the staff, the blunt end is resisted by the cellular substance, and thus we are warned to withdraw the instrument, and introduce it again into the groove. The instruments, which cut as they go in, make way for themselves; and the mischief is done before we perceive that they have taken a wrong direction. Where the incision is made

from before backwards, the parts yield and elude the instrument: a narrow cut and a consequent difficulty in removing the stone are the consequences. When the lithotome is employed, the neck of the bladder cannot escape the action of the blade, but must be divided in an extent corresponding to the degree of its aperture." p. 536, 537.

In London the two charities for the relief of persons affected with diseases of the eyes, have become firmly established, and have reached that extensive utility, which must very highly gratify the benevolent feelings of their patrons. The extraction of the cataract is practised at one of these institutions, and with greater success than it has ever met with, in any other hands. We cannot state the precise numbers of the fortunate and unsuccessful results, as we have no printed report to refer to; but we know, that the latter are so few, that the operation may be deemed certain of its object, unless some unfavourable circumstance is present. We need not state how complete a contrast to this representation would be exhibited in the fate of patients operated on in general hospitals. We cannot help indulging a wish, that the vast opportunities of practice, afforded by these charities, should be employed for the purpose of settling the hitherto disputed question, whether depression or extraction be the preferable operation. Among writers, the greatest authorities are decidedly in favour of the former; while the two celebrated oculists of this metropolis practise the latter exclusively. This apparent contradiction will surprise us less, when we reflect that the practice of the oculist depends entirely for its existence, as a distinct branch of the profession, upon this single operation; and, that if couching were introduced instead of it, any given surgeon would be able to cure cataracts as well as the most expert oculist. From these considerations we had great pleasure in hearing that the late Mr. Saunders, surgeon to the London infirmary for the eye, had practised a modification of couching in certain cases, and particularly that he had applied his method to very young subjects, in which extraction is altogether impracticable. This gentleman was on the point of communicating to the public his experience on the above mentioned subject, when an apoplectic attack suddenly removed

him from a situation at which the exertion of his talents was not less beneficial to others, than honourable to himself; just when he was beginning to receive the well-deserved reward of public fame and emolument. While we deplore the loss experienced on this occasion by the public and the profession, it is satisfactory to learn, that Mr. Saunders's manuscripts are nearly ready for the press, and that his work will appear without delay. In the mean time we may state, as a matter of general report, that the operation performed by Mr. S. on young children, consisted simply in puncturing and disturbing the cataract, without attempting to depress it. The opake substance, when its natural connexions are thus destroyed, will be removed by absorption. Our readers will be pleased to see Mr. Saunders's own statement of his proceedings; which we extract from a letter published in the report of the London infirmary, 1809. "In addition to the observations made on the former reports, and equally applicable to the present, there is one point on which I must beg the indulgence of expatiating; I mean the adaptation of an operation on the cataract to the condition of childhood, by which I have successively cured, without a failure, thirty-one persons born blind, many of them in infancy, and one even at the early period of two months. Reserving, for another occasion, the communication of the method which I pursue for the cure of very young children, I shall no farther compare it with extraction, than by observing, that extraction is wholly inapplicable to children, or only fortuitously successful. Those who on all occasions adhere to this operation, and have never turned their thoughts towards the application of means more suitable to this tender age, have been obliged to wait until the patient has acquired sufficient reason to be tractable: otherwise, when they have deviated from this conduct, the event has afforded little cause of self-congratulation. How great the advantage of an early cure, is a question of no difficult solution. Eyes originally affected with cataracts contract an unsteady and rolling motion, which remains after their removal, and retards, even when it does not ultimately prevent, the full benefit of the operation. A person cured at a late period cannot overcome this awkward habit by the utmost exertion of reason, or efforts of the will. But the

actions of the infant are instinctive. Surrounding objects attract attention, and the eye naturally follows them. The management of the eye is therefore readily acquired, his vision rapidly improves, and he will most probably be susceptible of education about the usual period."

Mr. Albers, of Bremen, having examined the eye of the codfish, Gadus Morrhua, L. finds that the sclerotica is composed of two coats; the outermost of which is membranous, while the innermost is horny, and divisible into several layers. The innermost of these layers is coated with a substance resembling spermaceti, which forms little cysts, containing water in their intervals. This separates the sclerotica from the outermost silvery coat of the choroides, which by maceration becomes divisible into two layers. The intermediate coat of the three principal ones, that occupy the place of the choroides in the eyes of fishes is composed in the cod of a retiform vascular texture, the trunk of which issues from the horse-shoe shaped tubercle peculiar to fishes, and the use of which is so difficult to explain. Some anatomists have taken this tubercle for a gland, others for a muscle; but it appears from the minutest research to be a convolution of larger bloodvessels, and Mr. Albers considers it as a sort of reservoir for the blood, a kind of rete mirabile.

In the 10th vol. of the Asiatic Researches Dr. Rene has given an account of the male plant, which furnishes the medicine generally called *colomba root*. This plant is indigenous on the eastern coast of Africa: a fact we believe never hitherto ascertained, although the Portuguese export the root in very considerable quantities from the Mosambique. An offset taken to Madras, and there planted, produced only male flowers, so that the genus is still uncertain.

The conclusions drawn by Mr. Davy in his late publication on the muriatic acid will serve to extend and enlighten the theory of chemistry to a greater extent than any of the brilliant discoveries formerly made by this illustrious chemist. They are as follows.

1st. That the oxymuriatic acid is a simple substance, which may be classed in the same order of natural bodies, as oxygen gas; being like oxygen determined to the positive surface in voltaic combinations, and like oxygen, combining with inflammable substances, producing heat and light.

2dly. That its combinations with inflammable bodies are analogous to oxides and acids in their properties and powers of combination, but they differ from them in being, for the most part, decomposable by water.

3dly. That hydrogen is the basis of the muriatic acid, and oxymuriatic acid its acidifying principle.

4thly. That the compounds of phosphorus, arsenic, tin, &c. with oxymuriatic acid, approach in their nature to acids, and neutralize ammonia and other acidifiable bases.

5thly. That the combination of ammonia with phosphorus, acidified by oxymuriatic acid, is a peculiar compound, having properties like those of an earth, and is not decomposable at an intense red heat.

6thly. That the oxymuriatic acid has a stronger attraction for most inflammable bodies, than oxygen; and that on the hypothesis of the connexion of electrical powers with chemical attractions, it must be highest in the scale of negative power, and that the oxygen, which is supposed to exist in oxymuriatic acid, has always been expelled from water or oxides.

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